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SOCIOLOGICAL

REVIEW

Empirical Assessment of Values and Ideologies *William A. Scott*

A Theory of Value *William R. Catton, Jr.*

A Theory for Intergroup Beliefs and Action *Louis Guttman*

Statistical Problems in Research Design *Leslie Kish*

The Geometric Interpretation of Agreement *W. S. Robinson*

Social Class and Posthospital Performance *Howard E. Freeman
and Ozzie G. Simmons*

Social Class and Parental Authority *Melvin L. Kohn*

Sex Temperament of Alcoholics and Moderate Drinkers
Frederick B. Parker

Cruelty, Dignity, and Determinism *Gwynn Nettler*

Review Article: "Models of Man" *Frederick Mosteller*

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EMPIRICAL ASSESSMENT OF VALUES AND IDEOLOGIES *

WILLIAM A. SCOTT

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Procedures have been developed for assessing values and ideologies of social groups by interviews or questionnaires administered to their members. These involve content analysis of responses to standardized questions concerning traits which the subject admires in other people. Although the procedure provides only a crude measure of the presence or absence of individual subjects' values, preliminary evidence on reliability and validity indicates that it is adequate for an initial overview of the salient values within a group. The measures were used to assess the values and ideologies of three different samples, representing adult residents of a university community, students at the university, and students at a minority sect religious college. Results are interpreted in the light of common assumptions about the cultures of these groups.

MUCH has been written about the ideology of American culture and of various subcultures within it. In the early days of the nation, Tocqueville¹ saw Americans as preoccupied with freedom, equalitarianism, and physical well-being. The industrial revolution and the rise of capitalism were presumably associated with the Protestant ethic, embodying the ideals of individualism, hard work, and self-denial.²

* The research reported here represents the first phase of a long-term study of personal values, supported initially by the Foundation for Research on Human Behavior (Ann Arbor, Michigan), and subsequently by the National Institutes of Health. The writer is also indebted to the following colleagues and students for their contributions to various phases of the interview survey project: Judson Pearson, John Dackawich, Eleanor Crow, John Meyer, and Charles Miller.

¹ Alexis de Tocqueville, *Democracy in America* (English translation), New York: Knopf, 1945. (Original publication, Paris: Librairie de Charles Gosselin, 1836.)

² R. H. Tawney, *Religion and the Rise of Capitalism*, New York: Harcourt Brace, 1926; and Max Weber, *The Protestant Ethic and the Spirit of Capitalism* (English translation), London: Allen and Unwin, 1948.

Recently Fromm, Riesman, and Whyte³ have noted the emergence of a marketing or other-directed personality orientation, said to be supplanting the ideology of the Protestant ethic. Mead⁴ characterizes Americans as at once idealistic and pragmatic; while Gorer⁵ stresses their generosity, hatred of authority, and concern with interpersonal relations and the acquisition of money.

Such characterizations of cultural ideology tend to highlight presumed modal patterns while de-emphasizing the diversities which surround them. Certain diversities are treated by Kluckhohn,⁶ who points out that hetero-

³ Erich Fromm, *Escape from Freedom*, New York: Farrar and Rinehart, 1941; David Riesman, *The Lonely Crowd*, New Haven: Yale University Press, 1952; and William H. Whyte, *The Organization Man*, New York: Simon and Schuster, 1956.

⁴ Margaret Mead, *And Keep Your Powder Dry*, New York: Morrow, 1942.

⁵ Geoffrey Gorer, *The American People: A Study in National Character*, New York: Norton, 1948.

⁶ Florence Kluckhohn, "Dominant and Variant Value Orientations," in Clyde Kluckhohn, Henry A. Murray, and David M. Schneider, editors, *Personality in Nature, Society, and Culture*, New York: Knopf, 1955.

geneous cultures are characterized by variant, as well as dominant, value orientations.

But whether cultural ideologies are viewed as unitary or diverse, there is usually considerable ambiguity concerning the empirical bases of such generalizations. Value systems may be read from official documents and pronouncements, such as the Declaration of Independence or the President's State of the Union Message. They may be inferred from content analyses of communications media or from art forms and other cultural artifacts. Or they may be derived by questioning members of the society about the personal values that they hold sacred. Whatever the procedure, the selection of relevant data for value inference involves a sampling problem. What population of documents is to be taken as embodying the overt values of the culture? What population of communication media or art forms is definitive of the group's culture? What population of persons may appropriately be regarded as validators of a society's ideology? Given definitions of these populations, what sorts of sampling procedures should one use to ensure adequate representation of the intended population? Should newspapers be sampled disproportionately according to readership? Should varieties of an art form be differentially weighted depending on the size of their audience?—or on experts' judgments of their importance to the culture? Should a population of individuals be sampled in random fashion, with equal weight given to all informants? Or should respondents be differentially represented according to their social visibility?

Studies of cultural ideology have given little explicit attention to such problems of research method. Generally the population and sampling procedure are not specified; and the analyst's method of integrating data on which inductions are based is left inexplicit. An astute interpreter may derive insights which are validated by their correspondence either with the reader's common-sense judgments or with another analyst's interpretation. In either case, the basis of the validation is usually as mysterious as the original interpretation. It is not unusual, under such circumstances, for independent investigators to reach quite divergent in-

terpretations of cultural patterns.⁷ These divergencies may be attributed to at least two sources: different investigators may collect their data from quite different populations of events; and the inferred cultural patterns are highly dependent on the interpretive work of the investigator, which is not duplicated from one researcher to another.

Confusion of interpretations concerning cultural ideologies is also brought about by the theoretical problem of what is meant by "culture." From the standpoint of a culturologist (many students of cultural anthropology appear to share this viewpoint), culture represents a set of externalized ideas or other products of work and thought. Individuals may serve as culture-carriers, but cultural elements themselves are viewed as having a separate existence, functioning within a deterministic system composed only of other cultural elements. From this point of view, the appropriate objects of analysis for cultural investigations are such products as language, art forms, and material artifacts which are not directly dependent for their current existence on the participation of particular individuals (although individual members of society, in a sense, may have been responsible for their origins).

Another definition of culture, widely held among social psychologists, refers to ideas which are held in common by members of a society or social group. Although these cultural elements may display the normative properties characterized by Durkheim as "exteriority" and "constraint," their existence as cultural facts ultimately depends on their being shared by a significant proportion of persons in society. Thus cultural values and ideologies are functions of similarities in the cognitive structures of individuals.

Clarity of discourse, it seems, requires that these two definitions of culture be kept conceptually distinct. And each should be measured by operations appropriate to its definition. Culture as a system of shared meanings should be assessed by questioning individuals and determining the extent of agreement among them. Culture as an ex-

⁷ See, e.g., J. J. Honigman, *Culture and Personality*, New York: Harper, 1954, pp. 100-101.

ternal system of ideas and products should be assessed by direct observation and interpretation of these products. But descriptions of cultural ideologies have often confused these two, interpreting data relevant to one definition as if they referred to the other. Thus, members of a particular national group may be assigned traits, such as glorification of feminism, on the basis of content analysis of communication media. It would seem preferable to obtain independent measures from cultural products and from members' cognitive structures, and to treat their degree of correspondence as a matter for empirical investigation. Instances of discrepancy could be every bit as interesting as instances of congruence. In any case, congruence should not be assumed in the absence of evidence from independent sources.

These considerations form the theoretical and methodological context of the research reported below.

ASSESSING MORAL IDEALS OF INDIVIDUALS

A *personal value*, or *moral ideal*, has been defined as a particular individual's concept of an ideal state of affairs or relations among people, which he uses to assess the "goodness" or "badness," the "rightness" or "wrongness" of actual relations which he observes.⁸ Instruments for assessing "values" have been developed by Allport and Vernon and by Morris,⁹ but these were considered inappropriate for the present investigation because they are constructed so as to measure only a limited number of value orientations. Moreover, the Allport-Vernon scale appears more like a test of object-preferences than a test of personal values in the present sense; most of its items do not require moral evaluation of courses of action, but simply selection among them.

In order to avoid artificial restriction of the range of subjects' evaluative responses,

⁸ William A. Scott and Stephen B. Withey, *The United States and the United Nations: The Public View*, New York: Manhattan Publishing Co., 1958, p. 226.

⁹ Gordon W. Allport, P. E. Vernon, and Gardner Lindzey, *Study of Values*, Boston: Houghton Mifflin, 1951; Charles Morris, *Varieties of Human Value*, Chicago: University of Chicago Press, 1956; and Charles Morris and L. V. Jones, "Value Scales and Dimensions," *Journal of Abnormal and Social Psychology*, 51 (November, 1955), pp. 523-535.

an open-question measure of moral ideals was developed for use either in interview surveys or in questionnaires. Although providing, at best, a crude assessment of any particular personal value, it has the advantage of permitting expression of a limitless variety of values, so that the researcher can determine empirically just what standards of "goodness" or "rightness" are expressed voluntarily by members of any given population. For more precise assessment of particular moral ideals, one can develop multiple-item scales by the Guttman, Likert, or Thurstone techniques.

Populations and Samples. Subjects were selected from three different populations: (a) adult residents of "Mountaintown," a predominantly upper-middle class suburban community of approximately 30,000 inhabitants; (b) students at State University in Mountaintown; and (c) students at Fundamental College, a minority sect college in the Midwest. Samples from populations (a) and (c) were selected by probability sampling procedures, and 80 per cent or more of the designated respondents were contacted.¹⁰ Their moral ideals were assessed by non-directive interview. Population (b) was not sampled in representative fashion, but students in a large general psychology class (mostly sophomores) were studied. Their moral ideals were assessed by a questionnaire closely similar to the interview questions used on the other two populations.

Measures of Moral Ideals. Those parts of

¹⁰ The probability sample of Mountaintown was obtained in multi-stage fashion from a systematic selection of blocks, then dwelling units, then adults within the designated D.U.s. (See Leslie Kish, "Selection of the Sample," in Leon Festinger and Daniel Katz, editors, *Research Methods in the Behavioral Sciences*, New York: Dryden, 1953; and Leslie Kish, "A Procedure for Objective Respondent Selection within the Household," *Journal of the American Statistical Association*, 44, [September, 1949], pp. 380-387.) Hotels, large rooming houses, and dormitories were excluded, since a dwelling unit was defined as a residence with private cooking facilities. This procedure eliminated a large proportion of the student population, but included those 21 years of age and over, living in private apartments and houses. No substitutions were permitted, and repeated call-backs were required to obtain interviews with the designated respondents. The sample of Fundamental College was drawn systematically from the list of students provided by the college office of admissions.

the interview schedule dealing with moral ideals were exclusively of the open question type, in which the interviewer attempted to record verbatim the respondent's answers. In accordance with the definition of a moral ideal as an abstract standard for judging concrete actions, the open questions were framed with a view toward eliciting standards of conduct.¹¹ In the State University sample moral ideals were assessed by mimeographed questionnaires on which subjects answered two questions comparable to those used in the interview.¹²

Responses, as recorded by the interviewer or by the subject, were subsequently coded by the research team.¹³ Since the

categories of moral ideals were not designated in advance, it was necessary to develop them empirically by perusal of the first fifty interviews returned, with provision for adding categories as necessary. Eventually sixty discriminable categories were established, but only thirteen of these were used with sufficient frequency (thirty or more respondents in the Mountaintown sample) to justify tabular analysis. By combining certain of the low-frequency ideals into more general groupings, it was possible to obtain a set of eighteen categories for correlational analysis of the Mountaintown data. (Three more were subsequently added for the other two populations.) A brief description of each is presented in Table 1. It must be emphasized that these eighteen moral ideals cannot be regarded as a general typology applicable to all populations. They are valid only for Mountaintown at the time of this study (Spring, 1957), and depend on the particular assessment procedures employed here.

Reliability of the Measures. Reliability of the measure of moral ideals was assessed in two ways: first, by the extent to which two different coders agreed in inferring a particular ideal from what the respondent reported; and second, by the degree to which respondents mentioned the same moral ideals in two successive interviews.¹⁴ The figures in the second column of Table 1 report a

¹¹ After a brief introductory and not overly informative explanation of the general mission of the interviewer, the respondent was asked: "I wonder if you would think about your various friends and pick out two that you admire most." "Now, what is it about the first person that you particularly admire?" "Anything else?" "Now what is it about the second person that you particularly admire?" "Anything else?" "Now, would you think of a couple of people whom you have very little use for. What about the first person would you say is especially bad?" "Anything else?" "What about the second person would you say is especially bad?" "Anything else?" The purpose of these "warm-up" questions was to encourage the respondent to think about people in an evaluative framework—which many are not accustomed to doing. There followed the questions from which the respondent's moral ideals were assessed: "So far I have asked you about specific people. Now I have a more general question. What is it about any person that makes him good?" "Anything else?" "What kinds of things about a person would make him especially bad?" "Anything else?" And for each item mentioned under "good" and "bad": "Why would you say that is good (or bad)?"

¹² The questions were: "Think about the various people you admire. What is it about them that you especially admire? In other words, what about any person makes him especially good?" "Now think about some people whom you have very little use for. What about them is especially bad? In other words, what about any person would make him bad?"

¹³ Answers to the question about "what makes anyone especially good" were coded by attempting to form reasonably clear descriptions of the implied ideal trait. Traits which subjects regarded as "especially bad" were coded into the same set of categories as the positive traits, by attempting to infer opposites of the disliked qualities. Such inferences and attempts to reduce respondents' varied expressions to a small number of categories meaningful to the analyst undoubtedly resulted in some oversimplifications and distortions.

¹⁴ A random sample of fifty interviews was coded twice—first by the original coder (any of five different persons), and second by the writer several months later. The reliability of coding each moral ideal was separately assessed. If both coders agreed that the respondent either did or did not mention a particular ideal, this was counted as an agreement. The crude percentage agreement was corrected by taking into account the amount of agreement that would be expected on the basis of chance, given the particular response frequency. (See William A. Scott, "Reliability of Content Analysis: The Case of Nominal Scale Coding," *Public Opinion Quarterly*, 19, [Fall, 1955], pp. 321-325.) Approximately three months after the original interview, the same fifty respondents included in the check-coding were re-interviewed, using the same questions as before. The interviewer was a different person in all cases. Agreement between the first and second interviews was measured in the same way as indicated above: If a respondent mentioned a particular value during both interviews, or did not mention it during either, this was counted as an agreement. The reliability index was computed to indicate the percentage agreement in excess of chance.

TABLE 1. CODING RELIABILITIES AND RESPONSE STABILITIES OF MORAL IDEALS PROFESSED BY MOUNTAINTOWN RESIDENTS

Moral Ideal	Coefficients of Agreement (π)	
	Between Two Coders on Pre-test	Between Pre- and Post-test
1. Self-control; abstinence; cleanliness; orderliness †	.90**	.71**
2. Religiousness; belief in God; church-going	.86**	.53**
3. Hard work	.81**	.38*
4. Honesty	.80**	.40**
5. Intelligence; mental development; intellectuality	.79**	.51*
6. Humility; democratic egalitarianism	.77**	.00
7. Genuineness; sincerity	.75**	.16
8. Happiness; optimism; sense of humor; easy-going nature †	.71**	.37*
9. Loyalty to family or friends †	.70**	.11
10. Fairness; objectivity; universalism †	.67**	.00
11. Dependability; responsibility	.60**	.38*
12. Love of people; kindness toward people	.56**	.10
13. Social interaction skills; ability to get along with people	.56**	.26
14. Friendliness; affability	.43*	.21
15. Integrity; adherence to one's ideals	.30	.39
16. Respect for individual dignity; tolerance for differences	.25*	.19
17. Respect for authority; prestige; power; status †	.22	.16
18. Generosity; helpfulness	.17	.16
Mean inter-coder agreement and mean stability	.57	.26

† Combined of two or more originally distinct categories with frequencies too small for separate analysis.

** $P < .01$.

* $P < .05$.

corrected index of inter-coder agreement, π , which represents the percentage agreement above chance. In general, these figures are reasonably high, indicating, at least for some of the value categories, that trained content analysts can agree on coding of the respondents' answers. The measures of response stability, from the first to the second interview, are reported in the third column of the table. It is apparent that reliabilities of respondents' reports are much lower than the reliabilities of judging their values from

a single interview. Either there was considerable change in moral ideals in the three-month interval; or the same set of ideals changed in relative saliences, so that some were readily mentioned one time and not the other; or the different approaches of the two interviewers elicited different responses to the same questions. Whatever the sources of instability in responses, clearly this method of assessment may not yield comparable results for some of the moral ideals from one interview to another later one.

Validity of the Measures. Since no independent measures of the moral ideals were available in this study, it was necessary to resort to validation by inference from correlations with other responses. If a person maintains moral ideals in the sense intended here, then he would be expected to use them to evaluate significant others in his environment. Therefore, it is reasonable to expect that criteria used in assessing other people should correlate with the criteria embodied in the moral ideals. At various points in the interview three different kinds of significant others were suggested: the respondent's acquaintances, his children, and the respondent's self.¹⁵ Responses to these questions about specific people were coded according to the same set of categories used to assess

¹⁵ The criteria used by the respondent for assessing his acquaintances were ascertained in the course of measuring his moral ideals, by the "warm-up" questions reported in footnote 11. Since these questions appeared just before those from which the personal values themselves were assessed, it is almost certain that some of the consistency in use of the categories is artificially high. Questions about the respondent's children and about himself were asked later in the interview, so there was little chance for contamination between these responses and responses to the general questions about moral ideals.

Criteria applied to the respondent's children were assessed by the following kind of questions: "Suppose you wanted to raise your children to become the finest possible people. What are the important things you would try to teach them?" (Appropriate variations in the question were provided for childless respondents and for those whose children were adults.) Criteria by which the respondent assessed himself were determined by the questions: "Now that you've told me what you admire in other people, I wonder if you'd answer a couple of additional questions about yourself. Most people have some things about them that they're rather satisfied with. What about yourself do you like?" "What else?" "What about yourself do you not like?" "Anything else?"

TABLE 2. RELATIONS BETWEEN MORAL IDEALS AND STANDARDS USED FOR JUDGING ONE'S ACQUAINTANCES, ONE'S CHILDREN, AND ONESELF

Moral Ideal	Association with Standards for:		
	Acquaintances	Children	Self
1. Self-control and the like		(*)	
2. Religiousness	**	**	**
3. Hard work	**	**	(*)
4. Honesty	**	**	**
5. Intelligence	**		**
6. Humility	**		
7. Genuineness	**	**	
8. Happiness	**		**
9. Loyalty	.		.
10. Fairness	**	**	.
11. Dependability	**	**	**
12. Love of people	**	**	
13. Social skills	**	**	
14. Friendliness			.
15. Integrity	**		*
16. Individual dignity	**	**	
17. Respect for authority	**		
18. Generosity	**	.	

** $P < .01$.* $P < .05$.(*) $P < .10$.

the general moral ideals. Hence one can determine the respondents' tendencies to use the same categories to describe admired people and good qualities in general (or, alternatively, their tendencies to use opposites of the favored general qualities to describe bad traits in themselves or others).

Results of these comparisons are reported in Table 2, where significant associations between general moral ideals and the criteria applied to the three categories of people are indicated by asterisks. From the table as a whole it can be seen that more often than not moral ideals tend to be applied as criteria for assessing oneself, one's children, and one's acquaintances. Those ideals for which significant associations do not appear tend to be the ones previously found (Table 1) to be unreliable or unstable. Thus one would expect that improvement in the assessment procedure would result in even greater tendencies for the moral ideals to be correlated with criteria for judging people.

CULTURAL VALUES IN THE THREE POPULATIONS

To the extent that many members of a particular social group give responses which

can be classified into a single category of moral ideals, one can say that a cultural value exists. It would be unusual, indeed, for an open question technique such as this to yield anywhere near 100 per cent classification within a single category, so one must be careful to interpret the response frequencies in relative terms. If specific questions had been framed for each of the categories, all of them would undoubtedly have elicited much higher rates of assent than were found in this study. But such a procedure would have been unwieldy in the present research context, and would inevitably have focused attention on particular values to the exclusion of others.

Cultural values of the three populations may be inferred from Table 3, which lists the proportions of respondents from each sample who professed each ideal. Although Mountaintown can not be regarded as representative of the entire United States, it is nevertheless interesting to note the distribution of responses (second column of figures) in the light of interpretations of "American character" based on other kinds of data. Evidently moral ideals associated with "people" and interpersonal relations rank high in popularity, in contrast with moral ideals related to individuality and self sufficiency. Ideals involving self-control are much more popular than those involving expression of impulse. Aside from self-control, the classical values of the Protestant ethic—hard work, stoicism, and achievement—are scarcely mentioned. The ideals of creativity, aesthetic sensitivity, and appreciation of natural beauty were found so rarely that they could not be profitably included in the tabulation. Respect for authority, prestige, and status (combined category) received less frequent mention than their antithesis, democratic equalitarianism (or humility).

Thus the picture of Mountaintown obtained from this interview assessment of moral ideals is suggestive of Riesman's "other directed" type of social character.¹⁶ The popular values in this town tend to be those which concern social relations rather

¹⁶ David Riesman, *The Lonely Crowd*, New Haven: Yale University Press, 1952.

TABLE 3. MORAL IDEALS IN MOUNTAINTOWN, STATE UNIVERSITY, AND FUNDAMENTALIST COLLEGE

Moral Ideal	Proportion Expressing the Ideal in:		
	State University (N = 295)	Mountaintown (N = 319)	Fundamentalist College (N = 47)
1. Love of people	35%	**	55%
2. Honesty	23	**	28
3. Individual dignity	8	**	26
4. Generosity	13	**	23
5. Self-control, etc.	24		55
6. Genuineness	27		17
7. Social skills	43	**	66
8. Friendliness	15		55
9. Dependability	13		17
10. Religiousness	1	**	49
11. Happiness	20		43
12. Fairness	7	**	15
13. Humility	16		43
14. Integrity	15		21
15. Hard work	13		28
16. Loyalty	4		21
17. Intelligence	31	**	9
18. Respect for authority	6		38
19. Individuality; self-sufficiency; independence	32	**	32
20. Achievement; striving to do well	24	**	6
21. Being liked by others	0		47

** $p < .01$ (by t-test) (refers to difference between percentages in adjacent columns).

* $p < .05$.

than relations with oneself, values related to the present rather than to the past or future, and values of impulse restraint rather than hedonism or power.¹⁷

The Mountaintown data may be used as a standard of comparison for the other two populations (first and third columns in Table 3). Significant departures of the student groups from the Mountaintown sample are indicated by asterisks between the appropriate columns of figures. The four values

which were relatively more popular in State University include social interaction skills, intelligence, achievement, and individuality. The first three of these correspond to the commonly held picture of the university as encouraging student orientations toward academic and social goals; academic endeavors are presumably stressed largely by the faculty, while concern for social skills is engendered within the fraternity and sorority groups. The relative prevalence of individualism as a value appears inexplicable on an *a priori* basis, unless one attributes it either to the adolescent years or to academically related pressures.¹⁸

Fundamentalist College presents a picture of striking contrast to that found in Mountaintown. Mentioned relatively more frequently in the former population are a large

¹⁷ The failure of a particular trait to appear in the list of popular moral ideals does not, of course, imply that individuals do not covertly harbor it. Under a respondent's profession of the value of respect for the dignity of the individual may have lurked an unexpressed desire for power and manipulation of people. But such a desire was not seen by a significant number of respondents as sufficiently admirable to advocate in reply to the interviewer's questions about traits that he admired in others. One must clearly note the distinction between a moral ideal (professed standard of conduct) and a motive (state of affairs toward which a person, consciously or unconsciously, strives). Within a particular individual the relation between these two may range from complete congruity to diametric opposition. The consequences of varying degrees of compatibility between motives and moral ideals presents an intriguing problem for subsequent research.

¹⁸ Instances in which the proportions in State University fall below those for Mountaintown should perhaps not be regarded too seriously at this point. While they may represent real differences between the values of students and townspeople, it is also possible that they are due in part to the difference in assessment techniques. The questionnaire filled out by students yielded a lower average number of moral ideals per person than did the interview used on the adult sample.

number of moral ideals: religiousness, self-control, respect for authority, humility (democratic equalitarianism), hard work, individuality, happiness, social interaction skills, friendliness, and being liked by others. Most of these differences can be attributed to three conditions: the strong religious orientation of the college, the need of a deviant group to maintain strong in-group ties, and the correlative need to resist pressures for conformity to out-group demands which are incompatible with its ideology. Such an interpretation leaves unexplained the relatively greater emphasis on happiness as a value, and does not account for the failure of generosity, love of people, integrity, and loyalty to emerge as significantly more popular ideals. (The last three of these values show sample differences in the expected direction, but they are not large enough to warrant conclusions about population differences.)

It is apparent that this method of assessing cultural values indicates some substantial differences among the three populations tested. To a considerable degree these differences are consistent with what one might expect on the basis of generally held views of the two academic institutions. Although there are no systematic data to validate these views, casual impressions suggest that if independent measures of the *external* cultures of these institutions could be obtained, they would correspond with the data concerning *internal* cultures reported here.

Valid comparisons between the internal and external cultures would require systematic data from the latter; these might appropriately be obtained from public documents, pronouncements of leaders, and institutional routines. Within a well integrated institution which provides for adequate socialization of its members, one would normally expect to find good correspondence between the two, but cases of perfect congruence are probably rare. Directors of State University would perhaps be disturbed to find that only about a quarter of the students profess the value of academic achievement, while directors of Fundamentalist College might be equally distressed to discover that only half of their students appear to share the value of religiousness. Thus instances of discrepancy between external culture (ex-

pected by institutional leaders) and internal culture (actually shared by participants) could provide interesting points of departure for the study of cultural disequilibrium and change.

ASSESSMENT OF CULTURAL IDEOLOGIES

To the extent that moral ideals are woven into an inclusive and consistent ideology which is shared within a sizeable segment of the community, one would expect to find such relationships among the various ideals that the possession of one value would make the belief in certain others either more or less likely. Conversely, the absence of such interrelationships would suggest either that the various moral ideals may be maintained quite independently by the individual, or, alternatively, that whatever the degree of integration among values within a given individual, this particular mode of integration is relatively unique.

In order to determine the extent of shared value structures within Mountaintown, each moral ideal was related to every other, and the statistical significance of the relationships tested by Chi-square.¹⁹ The significance levels, reported in Table 4, suggest the presence of shared ideologies in some areas tapped by this measure. The table shows four or five clusters of values, each with some degree of coherence, as well as an occasional suggestion of repulsion of one or more of the other values.

First, there is a cluster (I) including honesty, integrity, and genuineness, indicating a certain amount of coherence and shared possession of an ideology, which might be labeled, too simply, "individual integrity." Next is a cluster (II), which could be called "trustworthiness," composed of the ideals of intelligence, dependability, and fairness. In the center of the matrix appears a tight, exclusive duo (III) of religiousness and self-control. This close association of two such distinct concepts suggests that common themes among religious values in Mountain-

¹⁹ While more refined techniques such as tetrachoric correlations or factor analysis would have been appropriate to interval-scale measures of the values, the sizeable error inherent in the present open-question measure (which assessed only presence or absence of a given ideal) appeared to render such elegance unprofitable.

TABLE 4. INTERRELATIONS AMONG MORAL IDEALS IN MOUNTAINTOWN

Moral Ideal	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1. Honesty			*	-						-								
2. Integrity			I **		(*)								-			(-)		
3. Genuineness			*	**		(*)												
4. Intelligence					**	**							(*)					
5. Dependability			(*)		**	II					--							
6. Fairness			(*)		**						(*)							(-)
7. Religiousness				--									-	(-)	-	-		
8. Self-control							III			(-)						-		
9. Love of people											*	**	(*)					
10. Individual dignity	-						(-)		*									
11. Generosity					--	(-)			**		IV							(-)
12. Friendliness							-		(*)				(-)					(-)
13. Happiness		-		(*)			(-)					(-)		**	(*)	(*)	-	
14. Respect authority													**	(*)				
15. Social skills							-						(*)	(*)		V		
16. Humility		(-)					-	-					(*)					
17. Loyalty													-					
18. Hard work						(-)				(-)	(-)							

** $p < .01$ (positive association)* $p < .05$ (*) $p < .10$ -- $p < .01$ (negative association)- $p < .05$ (-) $p < .10$

town may be predominantly of the inhibitive rather than the prescriptive sort. Such an interpretation is further substantiated by the observation that the religious ideal correlates positively with none of the other values; instead it correlates negatively ($p < .05$) with five of them.

The two clusters at the lower right of the matrix are less closely bound, but tentatively suggest the composite ideologies of "people orientation" (IV), composed of love of people, respect for the dignity of the individual, generosity, and friendliness, and "interpersonal opportunism" (V), including happiness, respect for authority, social interaction skills, and humility.

It is interesting to compare this matrix of value-interrelations found in Mountaintown with those obtained from State University and from Fundamental College. In Table 5, for State University, one can distinguish four clusters rather similar to those presented for Mountaintown. First, there is the "individual integrity" group (I), consisting of the same three ideals of honesty, genuineness, and integrity, plus a fourth—loyalty (in fact, loyalty is more prominent in the cluster for

State University than is integrity). Second, there appears an abbreviated "trustworthiness" cluster (II), including intelligence and fairness, but not dependability. Third, there is the "people-oriented" group (IV), made up of love of people, respect for the dignity of the individual, generosity, and of humility which replaces the friendliness ideal found in Mountaintown. Finally, a somewhat rearranged "interpersonal opportunism" cluster (V) appears, with the same values of social skills and happiness as those found in the Mountaintown cluster, but with self-control and friendliness replacing humility and respect for authority.

Instead of the religiousness-self-control pair (the former value was not mentioned frequently enough to appear in the State University matrix), an altogether new duo (VI) is formed of the ideals of hard work and achievement. The value of achievement did not appear to an appreciable extent in Mountaintown; there hard work was an isolated ideal. But in State University, hard work as a value is overshadowed by the achievement ideal (Table 3), and is closely associated with it.

TABLE 5. INTERRELATIONS AMONG MORAL IDEALS IN STATE UNIVERSITY

Moral Ideal	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1. Honesty		**	**		-														
2. Genuineness	**		*	(*)															
3. Loyalty	**	*																	
4. Integrity				(*)															
5. Intelligence	-																		
6. Fairness																			
7. Love of people																			
8. Individual dignity																			
9. Generosity																			
10. Humility																			
11. Social skills																			
12. Self-control																			
13. Happiness																			
14. Friendliness																			
15. Hard work																			
16. Achievement																			
17. Dependability																			
18. Respect authority																			
19. Independence																			

(See footnotes on Table 4.)

Finally, at the bottom of Table 5 is the ideal of independence or self-sufficiency, which was scarcely found in Mountaintown but is relatively prevalent at State University and, appropriately, correlates with virtually no other value.

The major differences between the ideologies of Mountaintown and State University may be summarized as follows: Mountaintown emphasizes the combination of religiousness and self-control to a much greater extent than does State University, while the latter places relatively more emphasis on the hard work-achievement combination. Whereas self-control is strongly linked with religiousness in the wider community, it is tied with social skills, happiness, and friendliness at the university. (It is noteworthy that, among these university students, self-control is apparently not seen predominantly as relinquishment of pleasure, but rather as a means toward, or concomitant of, pleasure itself—pleasure being closely linked with social interaction values.) Aside from these three major differences, the intercorrelation matrices from the two populations look about the same. This tempts one to infer that the ideological groupings found

at State University parallel those of the wider community represented by Mountaintown (although in different proportions, as is evident from Table 3). In fact, the university students are probably recruited for the most part from a population whose ideological structure is not greatly different from that of Mountaintown.

Fundamentalist College (see Table 6) displays quite a different ideological structure. Although there are marked associations among many of the ideals, there is little resemblance between these clusters and Mountaintown's. There is an almost recognizable derivative of the "interpersonal opportunism" cluster (Va), but this includes social skills, humility, and self-control, in place of the social skills, humility, respect for authority, and happiness values of the Mountaintown matrix. (It will be recalled that self-control was also a part of this cluster in State University; in this respect the two student populations are alike.) The values of happiness and friendliness are now found as parts of a larger, but somewhat loosely organized, group (Vb) which forms the dominant ideology of Fundamentalist College. This cluster comprises the seemingly

TABLE 6. INTERRELATIONS AMONG MORAL IDEALS IN FUNDAMENTALIST COLLEGE

Moral Ideal	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Authority		**	(*)										(-)			
2. Hard work	**			*	*											
3. Being liked	(*)			**												
4. Friendliness		*	**	Vb	*	*	(*)									
5. Happiness		*		*		**										
6. Independence			*	**												
7. Integrity			(*)					(*)								
8. Honesty							(*)		-							
9. Social skills									*	(*)			--			
10. Self-control								-	*	Va			-			
11. Humility			(-)						(*)							
12. Loyalty												(*)				
13. Love people	(-)											(*)				
14. Individual dignity									--	-				-		
15. Generosity														-		
16. Religiousness																

(See footnotes on Table 4.)

diverse ideals of respect for authority, hard work, being liked by others, friendliness, happiness, and independence. While the value of friendliness forms the most central link in this ideology, the latter is difficult to identify with a single phrase.

Two other major differences between this matrix and those previously observed suggest a marked difference between Fundamentalist College and the other two populations. First, only two value-clusters are found, in contrast with the five in Mountaintown and in State University. Second, there are relatively few negative correlations among the various ideals, compared with their profuseness in the other two matrices. Both of these characteristics suggest an ideologically more homogeneous group, with relatively little conflict among the moral ideals maintained by different members. In view of the population base from which Fundamentalist College students are recruited—nearly all are members of the same religious sect—this homogeneity, as well as the contrast with a community such as Mountaintown, can be readily understood.

Finally, it is noteworthy that the moral ideal of religiousness bears no relation to any of the other ideals, although it is professed

by about one half of the students. The independence of this value from the others suggests that it has no unique meaning for all group members. The college is presumably organized around a single, dominant religious theme, but the binding force of this theme is not supplemented—neither is it diluted—by inclusion in an ideology which embodies other popular and dominant moral ideals.

SUMMARY

This paper describes a method for assessing values and ideologies of a culture, when that culture is defined in terms of the shared psychological attributes of its members. The moral ideals professed by the individual participants in a particular social structure are determined by content analysis of their answers to open questions concerning traits which they admire or dislike in other people. A particular moral ideal is judged dominant (and hence a cultural value) to the extent that it is widely shared within the group. Cultural ideologies are inferred from inter-correlations among moral ideals within the group of respondents. Data concerning the values and ideologies of three different populations within the United States are reported,

together with interpretations of the differences found among them.

When respondents are selected as a representative sample of the larger population that constitutes a society or sub-society, this procedure would seem to be appropriate for determining the values and ideologies of the entire culture, provided that each individual's moral ideals are given equal weight in the total. However, if the group's culture is defined, not by reference to the shared psychological attributes of its members, but as an external system of ideas and cultural products, then the present assessment procedure is inadequate. Given the latter defi-

nition of culture, values and ideologies should be determined from direct assessment of relevant cultural products, such as language, art forms, mass communications, and other patterned behaviors. Unfortunately, studies of such external culture have provided little in the way of explicit guidance for the definition of the relevant population of events or for sampling in such a way as to represent faithfully that which they purport to measure. The provision of such explicit procedures would open the way for investigations concerning the degree of correspondence between culture, as psychologically defined, and culture as an extra-individual system.

A THEORY OF VALUE

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Persons and social organizations are frequently required to choose from among alternative desiderata. Such preferences are patterned, and this patterning is attributed to values. The theory of valuing presented here conceives the socialized human being as the center of several socially induced fields of force which attract him toward various desiderata. The magnitude of this attraction is a function of the proximity of the desideratum to the valuer in an n-dimensional value-space. The several force-fields are mutually inhibitory so that as the strength of one increases the strength of the others decreases. For each kind of field and for each person there is a more or less clearly defined class of desiderata by which the force of that field can be activated. Such classes of desiderata are defined for the person in the process of enculturation. A number of research hypotheses are embraced by this theory.

THE purpose of this paper is to state a theory of valuing behavior which is compatible with social-psychological theory and knowledge in the Cooley-Mead-Faris tradition and, by means of a model, to elucidate the concept of "value-space" which is fundamental in this value theory. The theory presented, in the main, is a synthesis of selected contributions by various sociological, psychological, and other value theorists.

SOME DEFINITIONS

Let a "desideratum" be anything some person desires at some time. It may be a material object, a social relationship, an item of information—in general, anything tangible or intangible denoted by the words "object of desire." "Valuing" may then be

defined as actions which show a person's intensity of desire for various desiderata,¹ or the amount of his "motivation"² to pursue them.

Every personality, and every social organization, is confronted at frequent intervals with the task of choosing among alternative desiderata. Energy expended to obtain one desideratum is not available for the pursuit of others. Preferences must thus be expressed among diverse objects. Typically, such preferences are not random, but patterned. The patterns they exhibit are rel-

¹ Stuart C. Dodd, "On Classifying Human Values: A Step in the Prediction of Human Valuing," *American Sociological Review*, 16 (October, 1951), p. 646.

² For one operational definition of "motivation" see L. L. Thurstone, "The Indifference Function," *Journal of Social Psychology*, 2 (May, 1931), pp. 141-142.

atively stable.³ Although such valuing behavior exhibits a certain amount of regularity within a given society or group, it may differ systematically from one society or group to another.⁴ This indicates that valuing (or preferring) is partly a function of socially acquired characteristics of valuers.

If preferential behavior exhibits certain regularities, to what may these be attributed? The noun "value" has usually been used to imply some code or standard which persists through time, and provides a criterion by which people order their intensities of desiring various desiderata. To the extent that people are able to place objects, actions, ways of life, and so on on a continuum of approval-disapproval with some reliability, it appears that their responses to particular desiderata are functions of culturally acquired values.

In investigating the values of a person or group, a sociologist studies "inferential constructs," rather than directly observable phenomena. These inferential constructs need not always resemble verbal statements that have been (or can be) made by the persons whose behavior is being studied. The sociologist may simply regard it as useful to try to infer from the patterned choices of persons or groups some "conception . . . of the desirable which influences the selection from available modes, means, and ends of action."⁵

The values inferred depend, of course, upon what sort of expressions of preference are used as data. If we study a person's actual preferences among an array of desiderata, any discerned regularities may cause

us to speak of that person's values; these are what Charles Morris has called "operative values." If we emphasize those preferences which seem to be based on a person's anticipation of the outcome of alternative behaviors, then the regularities to which we refer may be called his "conceived values." If we assume that, apart from his actual preferences and his anticipatory preferences, he *ought* to prefer certain things and disprefer others, then we are speaking of "object values"—those things which "are in fact" preferable, whether or not preferred, and whether or not conceived as preferable by the subject. As Morris notes, however, all three usages of the term "values" have in common some association with the concept of preference. Value may refer to the preferred, to what is conceived as preferable, or to the "actually" preferable.

According to Morris, "operative values" can be found through a study of preferential behavior. What is conceived to be preferable (conceived values) can be studied through the symbols employed in preferential behavior and the preferential behavior directed toward symbols.⁶ In effect, then, Morris operationally defines those variables which he calls "operative values" as factors to be found through an analysis of preferences among non-symbolic desiderata. "Conceived values" are operationally defined as factors to be found through an analysis of preferences among symbolic desiderata (that is, preferential behavior directed toward symbols employed in other preferential behavior). His category of "object values" remains without operational definition, perhaps implying that he relegates it to a non-empirical universe of discourse.

These operational definitions, and the general suggestion that axiology can be defined as the science of preferential behavior,⁷ may go a long way toward penetrating the confusion noted by Franz Adler in the literature on values. According to Adler the confusion arises because "value" is given several different meanings in the literature,

³ See William R. Catton, Jr., "Exploring Techniques for Measuring Human Values," *American Sociological Review*, 19 (February, 1954), pp. 49-55; William R. Catton, Jr., "A Retest of the Measurability of Certain Human Values," *American Sociological Review*, 21 (June, 1956), pp. 357-359; Hornell Hart, "A Reliable Scale of Value Judgments," *American Sociological Review*, 10, (August, 1945), pp. 473-481.

⁴ See Charles Morris, *Varieties of Human Value*, Chicago: University of Chicago Press, 1956, Chapter 3.

⁵ Clyde Kluckhohn, "Values and Value-Orientations in the Theory of Action: An Exploration in Definition and Classification," in Talcott Parsons and Edward A. Shils, editors, *Toward a General Theory of Action*, Cambridge: Harvard University Press, 1952, p. 395.

⁶ Morris, *op. cit.*, p. 12.

⁷ See Charles Morris, "Axiology as the Science of Preferential Behavior," in Ray Lepley, editor, *Value: A Cooperative Inquiry*, New York: Columbia University Press, 1949.

and it is not always clear which meaning a writer intends.⁸ But we cannot accept Adler's conclusion that the value concept is superfluous.⁹

According to Clyde Kluckhohn we should restrict the use of the term value to something combining the characteristics of "conceived values" and of "object values." He defines a value as "a conception of the desirable" that "is felt and/or considered to be justified."¹⁰ But if, as Morris seems to imply by having made no attempt to define them operationally, "object values" lie outside any empirical universe of discourse, how can we hope to study, by the methods of science, values so conceived?

RELATION OF DESIDERATA TO VALUES

Let us tentatively adopt Kluckhohn's definition, subject to revision as our analysis proceeds. If a value is "a conception of the desirable" then clearly we must study it, following Morris, by analyzing preferences among symbolic desiderata. Basic value-variables we might discover through such analysis need not, by logical necessity, bear any relation to the variables we might discover by analyzing the preferences of the

same population with respect to non-symbolic desiderata. As Adler points out, "All that the existence of a [verbally expressed] norm proves . . . is that there are people who want it to exist."¹¹ It does not prove that they want to, or will, comply with it. Compliance of behavior with stated norms, or consistency between operative values and conceived values, must be discovered empirically, rather than assumed *a priori*.

The full statement of Kluckhohn's definition, however, asserts such consistency. Values, he says, are conceptions of the desirable which influence selection from available modes, means, and ends of action. We can avoid the "hopeless and useless circularity"¹² of "explaining" behavior by merely "reading into" it the very values by which we purport to explain it if we make a sharper distinction between definition and hypothesis. To this end, it is useful to amend Kluckhohn's definition of value to read: *A value is a conception of the desirable which is implied by a set of preferential responses to symbolic desiderata.* (Unless modified by some adjective, the term "value" is hereinafter used in accord with this definition.) We may further assume, in keeping with modern social psychology, that these conceptions are socially acquired. Many of the cultural precepts (which, when internalized, become values as defined here) are customarily stated as absolutes. Our definition suggests therefore that we should study "conceived values" which are taken by the valuer to be "object values."¹³

The part of Kluckhohn's definition which we have omitted now becomes our first hypothesis. It may be stated as follows:

Hypothesis I: Socially acquired conceptions of the desirable (values) influence human choices among non-symbolic desiderata.

The rationale for this hypothesis lies in recognition of the fact that while, for purposes of conceptual clarity we can distinguish between symbolic and non-symbolic desid-

⁸ Franz Adler, "The Value Concept in Sociology," *American Journal of Sociology*, 62 (November, 1956), pp. 272-279. "Concepts of value can . . . be reduced to about four basic types: (A) Values are considered as absolutes, existing in the mind of God as eternal ideas, as independent validities, etc. (B) Values are considered as being in the object, material or non-material. (C) Values are seen as located in man, originating in his biological needs or in his mind. Man by himself or man in the aggregate, variously referred to as group, society, culture, state, class, is seen as 'holding' values. (D) Values are equated with actions."

Adler would study only type (D) which is very close to "operative values" as defined by Morris. Type (C) perhaps is equivalent to "conceived values." Types (A) and (B) together may be considered similar to what Morris calls "object values." Despite the apparent "researchability" of "conceived values" as operationally defined by Morris, Adler contends that Type (C) values are "inaccessible to the methods of the natural sciences (at the present state of our knowledge concerning internal mental and emotional phenomena). . . ." This statement seems unwarranted if we are willing to adopt the general position that preferential behavior, both symbolic and non-symbolic, can be observed, recorded, and studied.

⁹ See *ibid.*, p. 275 and p. 279.

¹⁰ Kluckhohn, *op. cit.*, p. 396.

¹¹ Adler, *op. cit.*, p. 278.

¹² See Adler's criticism of such circular analysis, *ibid.*

¹³ On the tendency of the members of most societies to reify their values, see William L. Kolb, "The Changing Prominence of Values in Modern Sociological Theory," in Howard Becker and Alvin Boskoff, editors, *Modern Sociological Theory*, New York: Dryden, 1957, p. 100.

erata, it must not be supposed that people in general are habitually fastidious about such distinctions. If the human personality is viewed as a single, coherent system, we must suppose that what goes on in one part of that system affects the behavior of other parts, so that "conceived values" may be expected to have some influence upon "operative values."

Two corollaries of this hypothesis may be stated:

Hypothesis 1-a: Significant correlations may be found, at any given time, between values and personal desires.

Hypothesis 1-b: Within an isolated social system, such correlations tend to increase through time. That is, there is a strain toward alignment of desiring with socially acquired values.¹⁴

However valid the above hypotheses may turn out to be, nevertheless desiring (and striving behavior) can hardly be considered wholly independent of the characteristics of desiderata. If Jones and Smith have internalized different values toward alcoholic beverages, Jones may order scotch and soda to quench his thirst while Smith orders a chocolate soda. The difference in their behavior reflects not only the difference in values but also their knowledge of the difference in alcoholic content of the two beverages.

More than the "objective" characteristics of the desideratum, however, are of relevance to the present theory—namely the characteristics perceived by the valuer. The teetotaler may readily consume a "spiked" chocolate soda if he is ignorant of its extra ingredient. It should be noted in this connection that the same behavior, in different cases, may have to be accounted for in terms of different values. That is, one person may be a teetotaler because of moral conviction that alcohol is evil; another may abstain because of a digestive problem. In both instances overt non-symbolic preferential behavior is a function of conceived values. And in both instances this behavior is also a function of the perceived characteristics of the desideratum. This discussion leads to a

further corollary of our initial hypothesis:

Hypothesis 1-c: The influence of values (as defined above) upon human choices among non-symbolic desiderata is conditioned by socially acquired knowledge of the characteristics of the desiderata.

Assuming the possibility of full knowledge of the "actual" characteristics of various desiderata, a valuer's perception of them still depends on certain other considerations. We are not likely to find that socially acquired conceptions of the desirable account for all of the variance in observable desiring. The formulation of hypotheses which may account for some of the residual variance requires examination of other variables that may affect the perception of desiderata by valuers.

THE CONCEPT OF VALUE-PERSPECTIVE

If Jones and Smith happen to be thirsty, a drink (alcoholic or otherwise) available in New York will be of no immediate value to them if they happen to be in Seattle. In their perspective, this desideratum is *perceived* as infinitesimal; the effort probably expended to obtain it (in preference to an "equal" desideratum at closer range) may be expected to be correspondingly infinitesimal. Their "motivation" to strive for it is approximately zero.¹⁵

Something like a value-perspective concept may underlie the behavior described by Hull's "goal gradient hypothesis." This hypothesis asserts that "there exists an excitatory gradient extending with positive acceleration according to the logarithmic law in an upward direction from the beginning of a maze to the reward box."¹⁶ In other words, the *apparent* value of an object is a function of its spatial proximity.

Dimensions other than physical space also seem to give rise to value-perspectives. Ask any P.T.A. member for what goals he is striving and he will probably name "better schools" among his aims. But, "other things being equal," it will very likely be easier to

¹⁴ See Allen L. Edwards, *The Social Desirability Variable in Personality Assessment and Research*, New York: Dryden, 1957, Chapter 3.

¹⁵ For a similar use of the term "motivation," see L. L. Thurstone, "Experimental Methods in Food Tasting," *The Psychometric Laboratory*, University of Chicago, 52 (June, 1950), p. 4.

¹⁶ C. L. Hull, "The Goal Gradient Hypothesis and Maze Learning," *Psychological Review*, 39 (January, 1932), pp. 42-43.

enlist his participation in a crusade for the betterment of the particular school in which his own children are enrolled than for the schools "on the other side of the tracks" or for a school attended principally by some minority group from which he remains aloof. The goal is perceived as less valuable when the relevant social distance is increased.

Similarly, time may be important. To a small child, the promise that he may "go to the show" tonight is almost invariably more desirable than the promise that he may go next week. Or, he may value a piece of candy *now* so strongly that the promise of several pieces "right after your nap" will not appease him. "The very young person will have time perspectives of limited range, while the successive age groups will show more extensive ranges. . . ." ¹⁷ Such perspectives, whatever their range, appear to be characteristic of human perception, adult as well as juvenile. Immediacy of interest tends to blind us in many instances, as Merton has noted, to the more remote consequences of an action which is consciously intended to secure only some proximate goal. ¹⁸

In general, then, for several dimensions, the *apparent* value (attractiveness) of an object may be a function of its proximity to the valuer. Hartmann draws the analogy with physical gravitation: ". . . the attraction of a goal is [directly?] proportional to its 'value' and inversely proportional to [the square of?] the 'distance' between the organism and the objective." ¹⁹ Laird, too, suggests a physical analogy: ". . . desire has to do with accelerations, not with simple velocities." ²⁰ Similarly, Parsons defines "concrete ends" as physical states of affairs that are desired. "Ends as a factor in action," however, are for Parsons the *departure* of such a state of affairs from the state antici-

pated by the valuer in case he does not act. ²¹ Or, expressed in hedonistic terminology by Laird, ". . . desire is not moved by pleasure as such, but by greater pleasure or lesser pain. In other words, desire is always *preference*—a transition to greater satisfaction, not the search for satisfaction taken simply and absolutely." ²² Since, in physical mechanics, "that which" produces an acceleration is a force, we may, by analogy again, speak of desire as a force, and employ for convenience the concept of a force-field. The gravitational analogy is one case of such a force-field conceptualization of valuing. By means of this analogy we arrive at the following hypothesis.

Hypothesis 2: When values are held constant, desiring (or "motivation") varies inversely with the "distance" (in an n-dimensional psychological space, or value-space) between the valuer and the desideratum.

This hypothesis asserts that at least part of the residual variance in desiring (after some variance has been accounted for by values) can be accounted for by the phenomenon of value-perspective. Valuing is seen then as a function of: (a) socially acquired values (force-fields surrounding the valuer in value-space); (b) socially acquired definitions of the characteristics of various desiderata; and (c) distances in some n-dimensional value-space between valuers and desiderata. Of course, the dimensions of value-space remain to be specified.

VARIATION OF FIELD STRENGTH

Magnetic fields provide a better analogue than gravitational fields for socially acquired values, since magnetism can be *induced* in certain previously non-magnetic objects. Therefore, we adopt, tentatively, an electromagnetic model for valuing behavior; and consider another important difference between magnetism and gravitation. This procedure, we believe, provides further insight into the processes of human valuing. The difference to be considered rests in the fact that, whereas gravitational fields are constant, in the case of electro-magnets the

¹⁷ L. K. Frank, "Time Perspectives," *Journal of Social Philosophy*, 4 (July, 1939), p. 297.

¹⁸ Robert K. Merton, "The Unanticipated Consequences of Purposive Social Action," *American Sociological Review*, 1 (December, 1936), pp. 901-902.

¹⁹ George W. Hartmann, "Immediate and Remote Goals as Political Motives," *Journal of Abnormal and Social Psychology*, 33 (January, 1938), p. 87.

²⁰ John Laird, *The Idea of Value*, Cambridge: University Press, 1929, p. 138.

²¹ Talcott Parsons, "The Place of Ultimate Values in Sociological Theory," *The International Journal of Ethics*, 45 (April, 1935), p. 285.

²² Laird, *op. cit.*, p. 138.

force-field may be "turned on and off," thus simulating intermittent desiring or non-constant motivation.

Desiring is intermittent, but it does not "go on and off" simply at random. Maslow has pointed out that desires cease to function as soon as they are gratified.²³ It is as if our electro-magnet were wired so that when it comes into contact with an object of attraction its field is automatically turned off. Gratification reduces motivation. Unlike the electro-magnet, however, the human valuer does not become inert when a desire is gratified; rather, a new desire comes into play. Here the physical analogy is not quite adequate for, so to speak, there are several kinds of qualitatively different force-fields, so arranged that when one is turned off another is turned on. Each of these fields exerts a force only on its own particular class of objects, and not on objects which may be influenced by another kind of field.

For the human valuer, Maslow envisages a hierarchy of "needs" in which the "lower" ones dominate if unsatisfied. The lowest unsatisfied level of needs at any given moment is dominant or "prepotent." Thus Maslow refers to a "hierarchy of prepotency,"²⁴ the levels of which are: (6) desires to know and to understand, (5) need for self-actualization, (4) esteem needs, (3) love needs, (2) safety needs, and (1) physiological needs. The meaning Maslow attributes to each of the above phrases need not detain us. The general idea is indicated by one extreme example: A hungry person may be obsessed with food, but only a relatively well-fed person is likely to become obsessed with working out a theory of valuing.

To sum up, a socialized human being may be conceived as if: (1) he were the center of several socially induced fields of force (corresponding, perhaps, to the levels in Maslow's prepotency hierarchy) which attract him toward—or, in the case of negative valuation, repel him from—various desiderata; (2) the magnitude of this attraction in each case is a function of the proximity of the desideratum to the valuer in an n-dimensional value-space; (3) within a given person the several force-fields tend to inhibit one an-

other so that as the strength of one increases the strength of the others decreases; (4) for each kind of field and for each person there exists a more or less clearly defined class of desiderata by which the force of that field can be activated, whereas it cannot be activated by objects outside this class; and (5) for each person, definition of the classes of desiderata activating each of his force-fields is largely a function of enculturation and social interaction.

The preceding paragraph summarizes a theory of valuing that can orient research. As formulated, the theory suggests only in general and abstract terms the effects to be expected from value-fields. In order to specify predictions of valuative behavior it would be necessary to know the mathematical structure of value-fields and to measure, among other things, the "distances" between valuers and desiderata. On the basis of such a theory, however, we can state in abstract terms a further hypothesis.

Hypothesis 3: When values and desideratum-to-valuer distances are held constant, desiring varies with the activation of levels in some prepotency hierarchy. In operational terms, this hypothesis states that the intensity of a person's desire for a given object varies systematically from time to time, and that this intensity at any particular time is a function of the continuously perceived similarity of the object to other objects strongly desired at that time.

MULTIDIMENSIONALITY OF VALUE-SPACE

In developing the concept of value-perspective, above, on which Hypothesis 2 is based, examples were given which suggest, plausibly, that value-space has at least the following dimensions: *physical distance*, *social distance*,²⁵ and *remoteness of the desideratum in time*.²⁶ Value-space, then, is probably multidimensional. Moreover, it is likely that more than these three most "obvious"

²³ George W. Hartmann, "Pacifism and Its Opponents in the Light of Value Theory," *Journal of Abnormal and Social Psychology*, 36 (April, 1941), p. 164.

²⁶ See Kurt Lewin, *Field Theory in Social Science*, New York: Harper, 1951, p. 34; as well as Hartmann, "Immediate and Remote Goals . . .," *op. cit.*; and Frank, *op. cit.*

²³ A. H. Maslow, "A Theory of Human Motivation," *Psychological Review*, 50 (July, 1943), p. 393.

²⁴ *Ibid.*, p. 394.

dimensions are required to depict value-fields adequately.

Another plausible dimension, that of *probability*, may be traced back to the thinking of Jeremy Bentham.²⁷ This dimension may be expressed as follows: the more probable a valuer considers his attainment of a given desideratum, the more strongly attracted he will be to that desideratum. Probability need not be regarded as an attribute of the desideratum, but of the valuer's relation to the desideratum. It is thus appropriately considered a dimension of value-space.

Somewhat similar conceptually to the probability dimension is the dimension of *irrevocability*, suggested by Hartmann.²⁸ To the extent that the valuer perceives a particular opportunity for obtaining a certain desideratum to be his *only* opportunity, its attractiveness for him at that time is enhanced. The dimension of irrevocability, then, refers to the tendency of the valuer to perceive a given object on a "get it now or never" or "opportunity knocks but once" basis. It has to do with the *improbability* of recurrent access to a goal.

One further dimension completes this tentative list of the coordinates of value-space. In the writer's experience and on the basis of general knowledge of contemporary American culture, it seems that people are more attracted to an object if they are in a position to believe that they have chosen it freely, uncoerced either by edicts or by such circumstances as the physical absence of alternative choices. A person's belief in this respect may have little to do with the actual characteristics of the desideratum, and so this variable also may be regarded as a dimension of intervening value-space. We call it the dimension of *free selectability*.

If we imagine that valuers are in motion in value-space, moving toward positively attractive desiderata and away from negative ones, a relation between dimensions is suggested. Perry suggests that something is perceived as more valuable the more "inclusive" it appears to be.²⁹ But in terms of

our model, inclusiveness subsumes two relations between dimensions, "subsidiation" and "congruency," as we term them here. Pointing to the obvious fact that a goal may be pursued in order that it in turn may be used as a means for the attainment of some more ultimate goal, Cattell has suggested the "subsidiation" of one desideratum to another.³⁰ In value-space, then, desiderata may vary in the degree to which they appear to lie directly *along the path* from the valuer to other desiderata. In similar fashion, they may vary in their proximity to a vector extending outward from the valuer directly away from such other goals as he is currently pursuing. Two desiderata which "pull" a valuer in "opposite directions" are incongruent. Thus a particular object may be characterized as more or less *congruent* with other desiderata.³¹ This reasoning leads to two additional hypotheses.

Hypothesis 4: A valuer's responses to sets of subsidiated desiderata are more predictable than his responses to sets of independent desiderata.

Hypothesis 5: A valuer's responses to sets of congruent desiderata are more predictable than his responses to sets of independent desiderata.

Our list of hypothetical dimensions now includes the following: (1) spatial distance, (2) social distance, (3) remoteness in time, (4) probability, (5) irrevocability or, perhaps, "permanence," and (6) free selectability.³² The procedures necessary for determining experimentally the actual dimensionality of value-space are not specified in this paper. However, by assuming that value-space is multidimensional and that its dimensionality is of the order of six or more, one further hypothesis can be formulated.

Value-space (assuming that it "exists") is itself a socio-cultural product. It may ap-

²⁷ Raymond E. Cattell, *An Introduction to Personality Study*, London: Hutchinson, 1950, pp. 47 ff.

²⁸ Hartmann, "Pacifism and Its Opponents . . ." *op. cit.*, pp. 164-165.

²⁹ Elsewhere I have listed seventeen "dimensions," but it now appears that some of these variables may more properly be considered attributes of desiderata, rather than dimensions of the value-space between a valuer and a desideratum. See, however, *Propaganda Effectiveness as a Function of Human Values*, unpublished Ph.D. thesis, University of Washington, 1954, p. 68.

²⁷ See Laird, *op. cit.*, p. 34; and Hartmann, "Pacifism and Its Opponents . . ." *op. cit.*, p. 168.

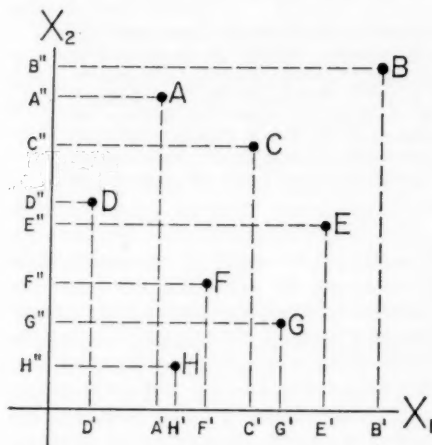
²⁸ Hartmann, *ibid.*, pp. 164-165.

²⁹ Ralph Barton Perry, *General Theory of Value*, Cambridge: Harvard University Press, 1930, pp. 615 ff.

pear to have somewhat different subsets of dimensions in different cultures and subcultures. If preferences among desiderata are a function not only of people's values, but also of the relative locations of the desiderata in value-space, then even with values held con-

accompanying figure, with the valuer located at the origin. Now suppose that the persons in one group have been enculturated so that they tend to perceive desiderata in terms of the one-dimensional space X_1 , while persons in another group tend to perceive the same desiderata in terms of a different one-dimensional space, X_2 . Thus, in the figure, the one-dimensional "projections" of these desiderata will have a different order of preference for these two groups— $D', A', H', F', C', G', E', B'$ for one group, and $H'', G'', F'', E'', D'', C'', A'', B''$ for the other group—although the *values* of the two groups may be the same. Thus:

Hypothesis 6: When values are held constant, the order of preferences among a set of desiderata may nevertheless vary from person to person or from group to group as a result of the failure of each person or group to be fully cognizant at all times of all the dimensions of value-space.



Schematic Representation of One-Dimensional Projections of Desiderata Located in Two-Dimensional Value-Space.

constant the structure of preferences may vary if the *effective dimensionality* of value-space varies from one person or group to another.

For explanatory purposes, imagine that value-space "really" consists of two dimensions, X_1 and X_2 . In this two-dimensional value-space the desiderata A, B, C, D, E, F, G, and H are located as shown in the

SUMMARY

We have presented six basic hypotheses and three corollary hypotheses, all related by the value-space concept and a magnetic model to a general theory of valuing. This theory, we believe, should enable social scientists to order many important data concerning preferential behavior. It also suggests two promising avenues of future research: (1) empirical attempts to differentiate the several mutually inhibitory force-fields in a prepotency hierarchy; (2) investigation of the mathematical structure of value-space.

A STRUCTURAL THEORY FOR INTERGROUP BELIEFS AND ACTION *

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The relation of an abstract theory to empirical research is one of the problems tackled by the metatheory of facets. A distinction is made between the structure of the universe of content of a theory and the statistical structure of the corresponding empirical observations. When there is a clear design for the universe, a special metatheory—like the contiguity principle—can be used to predict the statistical structure: the closer two variables are semantically, the closer they will be statistically. An empirical example is given to illustrate one type of content—intergroup behavior—for which the inference of statistical structure from semantic structure seems justified. Other special metatheories may be necessary for other types of universes of content.

THE relation between overt and covert behavior has attracted a great deal of discussion by social psychologists, as has also the relation between verbal and non-verbal behavior. Since such varieties of behavior are indubitably distinct from each other, it is not surprising that empirical correlations between them are not perfect, and indeed may be far from perfect. Each variety breaks down in turn into many subvarieties, which again may be far from perfectly related to one another—even when they refer to the same social object. To obtain very good predictions of one kind of behavior from others usually necessitates enlarging the framework to include even further types of variables.

It has been proposed elsewhere that the totality of behavior towards a social object be regarded as a universe which is divisible into subuniverses.¹ Recognizing that differential relations exist within and between varieties of behavior, the challenge to the social psychologist is to reveal what structural system, if any, underlies all these relations. Which subuniverses should be empirically closer to each other, and which more remote? A task of the social theorist is to provide an abstract framework whereby to define the subuniverses; the more adequately

it explicates the empirical correlations that ensue among the definitions, the better the framework. Comprehension of the multivariate system of the universe can lead to larger theories of relations with other universes, and thus to more and more perfect multiple correlations for each variety of behavior separately. The improved predictability will not depend on mere empiricism, then, but will be guided by a systematic social theory.

In a recent article, Bastide and van den Berghe² describe four types, or subuniverses, of content in connection with interracial behavior: *stereotypes, norms, hypothetical interaction, and personal interaction*. They present some interesting empirical findings in these areas, based on their Brazilian research, including a set of correlations among the four subuniverses. The purpose of the present paper is to suggest a structural theory for the observed interrelations among the four subuniverses. The proposed theory is structural in two different respects: its content and its statistical form. We first present a *semantic* framework within which to view the subuniverses, in terms of a facet design.³ From this is predicted a certain

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¹ Louis Guttman, "The Problem of Attitude and Opinion Measurement," in S. A. Stouffer et al., *Measurement and Prediction*, Princeton: Princeton University Press, 1954, pp. 46-59, esp. pp. 51-53.

² Roger Bastide and Pierre van den Berghe, "Stereotypes, Norms, and Interracial Behavior in Sao Paulo, Brazil," *American Sociological Review*, 22 (December, 1957), pp. 689-694.

³ On facet theory see Louis Guttman, "What Lies Ahead for Factor Analysis," *Educational and Psychological Measurement*, 18 (Autumn, 1958), pp. 497-515; Louis Guttman, "Introduction to Facet Design and Analysis," *Proceedings of the Fifteenth International Congress of Psychology, Brussels-*

statistical structure for the matrix of correlation coefficients. The empirical data are then examined to see whether or not they reveal the statistical structure predicted from the semantic structure. In the present paper, we shall not go on to consider how to enlarge the framework to an even more general social theory.

Proceeding from a semantic structure to a statistical structure seems essential for relating abstract social theory to empirical research.⁴ The present example may serve to illustrate how to go about doing this in a relatively simple, very limited, case. This is another purpose of the present paper. The same principles are being found useful for more complex social theories currently under construction.

DEFINITIONS OF THE FOUR SUBUNIVERSES

The four varieties of interracial behavior on which we are focussing are described briefly by Bastide and van den Berghe. For our present purposes, it is convenient to restate and to capsulize the descriptions. The following are definitions of the subuniverses⁵ of the four types:

I. Stereotype: *Belief* of (a white subject)

1957, Amsterdam: North-Holland Publishing Company, 1959, pp. 130-132; Uriel G. Foa, "The Contiguity Principle in the Structure of Interpersonal Relations," *Human Relations*, 11 (August, 1958), pp. 229-238.

⁴ The distinction between semantic and statistical structures, as different levels of systems of components, is made in Louis Guttman, "The Principal Components of Scalable Attitudes," and "A New Approach to Factor Analysis: The Radex," in P. F. Lazarsfeld, editor, *Mathematical Thinking in the Social Sciences*, Glencoe, Ill.: Free Press, 1954.

⁵ The subuniverses are listed here in a different order from that given by Bastide and van den Berghe, namely, in the order *a, b, d, c* of their notation. Their order was essentially arbitrary and played no particular role in their analyses, while our ordering has fundamental importance in our analyses, as will become apparent below. Our phrasing of definitions is often awkward, since our concern is with formalizing a certain conceptual structure rather than with literary elegance; the two are not always compatible. In the definition of "stereotype," we have even reversed the apparent emphasis from the more usual one of disparaging Negroes, again so as to maintain a formal structure comparable to that in the other definitions. We have changed the names slightly for the last two subuniverses from those given by Bastide and van den Berghe, for the convenience of the present analysis.

that *his own group* (excels—does not excel) in *comparison* with Negroes on (desirable traits).

II. Norm: *Belief* of (a white subject) that *his own group* (ought—ought not) *interact* with Negroes in (social ways).

III. Hypothetical Interaction: *Belief* of (a white subject) that he *himself* (will—will not) *interact* with Negroes in (social ways).

IV. Personal Interaction: *Overt action* of (a white subject) *himself* (to—not to) *interact* with Negroes in (social ways).

Italics and parentheses used in the above definitions are intended to indicate the semantic structure we are positing for intergroup behavior, and each will be explained.

All four definitions have in common the fact that they involve a white subject and Negroes. The phrase "with Negroes" occurs uniformly in each of the definitions, as does "a white subject." The intergroup behaviors of some pairs of groups other than whites and Negroes can be defined merely by replacing "white" and "Negroes" with the respective characterizations of the desired groups. It is convenient to think of specific groups, such as Negroes and whites, in developing our theory. The theory may be enlarged subsequently by letting the groups vary according to some principle.

The common or fixed elements indicate the universe of which the subuniverses are subsets. Since the subuniverses nevertheless differ among themselves, our first task is to ascertain the facets which determine those differences. Differences also exist *within* each subuniverse separately, for each in turn is a set of elements. So we shall also seek facets which differentiate within subuniverses.

THE STRUCTURE OF THE DIFFERENCES BETWEEN THE SUBUNIVERSES

The four definitions differ among themselves primarily on three facets,⁶ the ele-

⁶ The technical concept denoted here by "facet" occurs widely in the social and other sciences, as well as in mathematics, but strangely has never been given a standard name. Christening the concept by "facet" was formally proposed in Louis Guttman, "An Outline of Some New Methodology for Social Research," *Public Opinion Quarterly*, 18 (Winter, 1954-55), pp. 395-404, esp. p. 399. Prominent sociologists who have used the concept extensively in their own work, albeit rather informally, include Vilfredo Pareto, Leopold von Wiese, Talcott Parsons, Pitirim Sorokin, and Stuart Dodd, among others.

ments of which are italicized in the definitions given above. Each definition concerns a type of *behavior of a subject vis-a-vis* a type of *intergroup behavior* of a type of *referent*. Two kinds of behavior for the subject occur in the definitions: *belief* (a form of covert behavior) and *overt action*. Also, two kinds of referents occur: the subject's *group* and the subject *himself*. Similarly, two kinds of intergroup behavior are distinguished: *comparative* and *interactive*.

Thus, each of the three facets occurs as a dichotomy. It is helpful to list them and their elements in tabular form, assigning symbols to each for later use, as in Table 1.

in the rank order of the subuniverses suffices to divide the elements with subscript "1" from the elements with subscript "2." For facet A, the cutting point is between III and IV; for facet B, between II and III; and for facet C, between I and II. If we arbitrarily call subuniverse I the left end of the scale, we have the rank order $I < II < III < IV$, or more verbally: Stereotype < Norm < Hypothetical Interaction < Personal Interaction. The symbol "<" is used merely to signify that what is to the left of the symbol precedes what is to the right of the symbol in the rank order.

Scale structures have been found in various

TABLE 1. FACETS ON WHICH SUBUNIVERSES DIFFER

A=Subject's Behavior	B=Referent	C=Referent's Intergroup Behavior
a_1 =belief	b_1 =subject's group	c_1 =comparative
a_2 =overt action	b_2 =subject himself	c_2 =interactive

The capital letters A, B, and C denote the three facets, while the corresponding small letters with subscripts denote the elements of the respective facets.

Three dichotomous facets yield eight ($=2 \times 2 \times 2$) possible combinations of three elements each, one element from each facet. That is, the Cartesian product of the three facets, which may be denoted by ABC, is a set of eight profiles, each profile having three components. Each profile defines a different subuniverse. But we have defined above only four subuniverses, or only a subset of ABC. We may tabulate this subset explicitly as follows:

Subuniverse	Profile
I. Stereotype	$a_1 b_1 c_1$
II. Norm	$a_1 b_1 c_2$
III. Hypothetical Interaction	$a_1 b_2 c_1$
IV. Personal Interaction	$a_1 b_2 c_2$

A striking feature revealed by this tabulation is that the profiles form a perfect scale.⁷ For each facet, a single cutting point

kinds of empirical data—in ranking people, social institutions, political units, and so on. We now find the same structure in ranking abstract concepts. In the empirical cases, only approximately perfect or quasi-scales are the rule, which sometimes raises troublesome problems of how to handle scale error. On the nonempirical, conceptual level, truly perfect scales are not at all impossible, as exemplified above, with no problem of scale error.

THE MEANING OF THE RANK ORDER OF THE SUBUNIVERSES

Semantic meaning for the rank order requires exploration. According to scale theory, ordering the profiles also implies a formal ordering of the categories *within* each facet. The ordering $I < II < III < IV$ implies formally the three simultaneous orderings: $a_1 < a_2$, $b_1 < b_2$, $c_1 < c_2$. Do these further orderings have any common *semantic* meaning, formalisms aside? If not, the formal scale of the four subuniverses need have no clear semantic meaning.

A common meaning for the orderings can be suggested: they show in each case a progression from a *weak* to a *strong* form of

Studies in Scale Analysis, New Brunswick: Rutgers University Press, 1954; Guttman, "The Principal Components of Scalable Attitudes," *op. cit.*

⁷ While the literature on scale analysis has become rather widespread, some main references remain: Stouffer *et al.*, *Measurement and Prediction*, *op. cit.*; Louis Guttman, "The Cornell Technique for Scale and Intensity Analysis," *Educational and Psychological Measurement*, 7 (Summer, 1947), pp. 247-279, and also in C. W. Churchman *et al.*, *Measurement of Consumer Interest*, Philadelphia: University of Pennsylvania Press, 1947; M. W. Riley, J. W. Riley, Jr., J. Tobey *et al.*, *Sociological*

behavior of the subject vis-a-vis Negroes. "Belief" is weaker than "overt action" in being passive rather than active. Referring to the behavior of "subject's group" is weaker than the subject referring to "himself," insofar as the subject's relations with Negroes are concerned. "Comparative" behavior is weaker than "interactive" behavior since it does not imply social contact; a comparison is more passive than interaction.

Accepting this interpretation of the orderings within facets, we can say that the ordering of the subuniverses themselves also runs from weakest to strongest. "Stereotype" is the weakest form of intergroup subuniverse, "Personal Interaction" is the strongest form, while the other two subuniverses are intermediate in strength, in the indicated order.

THE OMITTED SUBUNIVERSES

What of the profiles from ABC that were not included among the original four definitions? These can define further subuniverses. Thus:

Two of the omitted profiles are $a_2 b_1 c_1$ and $a_2 b_1 c_2$. Each of these, according to its first two components ($a_2 b_1$), concerns overt action (a_2) of the subject with reference to the subject's group (b_1). Presumably, each implies that the subject should activate or stimulate his group—in one case as to how it compares itself with Negroes (c_1), and in the other, regarding interaction with Negroes (c_2). The first case might be called "Teaching" and the second "Preaching."

Assuming again that the simultaneous orderings $a_1 < a_2$, $b_1 < b_2$, and $c_1 < c_2$ hold, Teaching and Preaching are each stronger than Stereotype, and each is weaker than Personal Interaction. Furthermore, Teaching is weaker than Preaching. Indeed, we can spell out the structure of another scale with these new subuniverses, using the extreme subuniverses of the previous scale:

Subuniverse	Profile
I. Stereotype	$a_1 b_1 c_1$
II'. Teaching	$a_2 b_1 c_1$
III'. Preaching	$a_2 b_1 c_2$
IV. Personal Interaction	$a_2 b_2 c_2$

We have $I < II' < III' < IV$, moving from weakest to strongest.

The remaining two omitted profiles are

$a_1 b_2 c_1$ and $a_2 b_2 c_1$. According to their last two components ($b_2 c_1$), each of these concerns the comparison (c_1) of the subject himself (b_2) with Negroes as to desirable traits. The first profile might be called "Feel Superior" and the second "Act Superior." It would be interesting to pursue further the semantic meanings of these profiles, but this would carry us too far afield from our immediate purposes. In any event, again given the orderings $a_1 < a_2$, $b_1 < b_2$, and $c_1 < c_2$, both profiles are intermediate in strength to Stereotype and Personal Interaction, with "Feel Superior" being weaker than "Act Superior." Thus, we have another possible scale of subuniverse profiles:

Subuniverse	Profile
I. Stereotype	$a_1 b_1 c_1$
II". Feel Superior	$a_1 b_2 c_1$
III". Act Superior	$a_2 b_2 c_1$
IV. Personal Interaction	$a_2 b_2 c_2$

where $I < II' < III' < IV$.

Considering all eight profiles of ABC and the orderings $a_1 < a_2$, $b_1 < b_2$, and $c_1 < c_2$, three further scales are possible. They are indicated by the following three orderings, which hold simultaneously with the orderings given above:

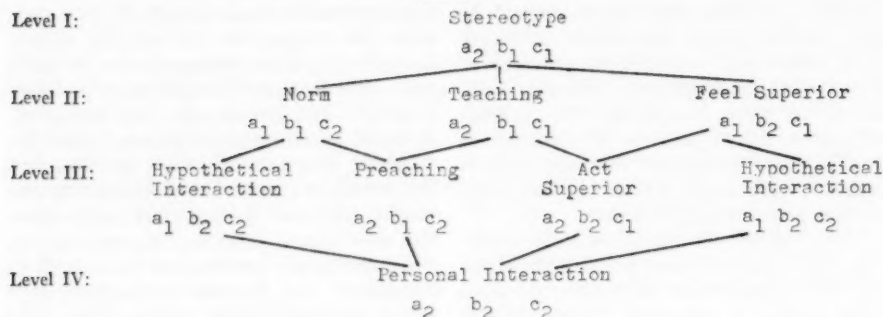
$$\begin{aligned} I < II < III' < IV, \\ I < II' < III' < IV, \text{ and} \\ I < II'' < III < IV. \end{aligned}$$

THE TOTAL UNIVERSE

No further scales are possible beyond the six listed above, given the orderings $a_1 < a_2$, $b_1 < b_2$, $c_1 < c_2$ for the three facets. One way of bringing out this limitation is to portray the entire universe as a partly ordered set, as in Figure 1. The connecting lines between "levels" in the figure show the pairs of profiles between which the inequality relation " $<$ " holds. If abc and $a' b' c'$ are any two different profiles, then $abc < a' b' c'$ if and only if simultaneously $a \leq a'$, $b \leq b'$, and $c \leq c'$.

In a scalable series of levels, each profile differs from its predecessor on only one facet. Starting with Stereotype as Level I, this allows for only three possible profiles for Level II, for there are only three subscripts in Stereotype that can be raised from a "1" to a "2." Since each profile in Level II has

FIGURE 1



only two subscripts of "1" that can be changed to a "2," there are only two ways that a profile can move to Level III, so each has two lines branching down from it. For convenience, we have listed Hypothetical Interaction twice in Level III in the figure in order to avoid criss-crossing lines. There are only three profiles possible at Level III, since there are only three ways of having one subscript equal to "1" and the remaining two equal to "2." The scales are the six continually descending possible pathways from Stereotype to Personal Interaction in the diagram, along the lines running from level to level.

The universe is only partly ordered, for many pairs of profiles are not comparable. For example, we cannot say that the inequality " $<$ " holds between Norm and Act Superior, in either direction; while the latter exceeds the former on facets A and B, the reverse is true on facet C. Any two profiles of the same level or noncomparable for similar reasons.

The concept of levels can nevertheless be justified. Both Norm and Teaching are intermediate to Stereotype and Preaching, or are between Levels I and III. Similarly, Teaching and Feel Superior are intermediate to Stereotype and Act Superior, and in this sense are also of the same level. Norm and Feel Superior are similarly bounded. Thus Norm, Teaching, and Feel Superior are bounded pairwise into similar intervals, and in this sense are of the same level. The same kind of justification holds for Level III for the three profiles listed there.

FACETS THAT DIFFERENTIATE WITHIN SUBUNIVERSES

Having completed the differentiation between the subuniverses, what about differences within subuniverses? Each subuniverse is in turn a set of behaviors, differing along three further facets which we label V, P, and R, respectively.

V refers to the set of all *variants* associated with the elements of facet C. While c_1 denotes comparative intergroup behavior, it does so only in a generic way, and there are many possible specific examples of such behavior. Indeed we have focussed—according to our definition of Stereotype—on "desirable traits." Now "desirable traits" subsume a rather large set, whose elements include foresight, suggestibility, self control, and intelligence, to mention only a few of the forty-one traits used by Bastide and van den Berghe (which in turn are but a sample from a much larger possible set.) Similarly, c_2 , or interaction, is a generic term for "social ways" of behaving which include playing together, visiting, intermarrying, and so on.

If V_1 designates the set of all "desirable traits" for comparative behavior, and V_2 the set of "social ways" of interactive behavior, then V is the union of these two sets.

Accordingly, each subject has many Stereotypes, for example—as many as there are elements in V_1 . This is indicated by the insertion of parentheses around "desirable traits" in the definition of Stereotype. The expression in parentheses is a variable, in the sense that it can be replaced by the

name of a particular trait (such as "fore-sight") so as to yield a particular stereotype on that trait. For the same reason, the expression "social ways" is enclosed in parentheses in the definitions in which it occurs, to show that names of particular "ways" may be substituted to yield specific forms of interaction.

The possible variants of behavior are further to be modified by their *degrees of favorableness* toward Negroes. The stereotype a subject may have of Negro intelligence, for example, may range from very favorable to very unfavorable. This range is indicated within the parentheses (excel—not excel) in the definition of Stereotype. Similarly, Norms, Teaching, Personal Interactions, and so on may each range from very favorable to very unfavorable. The expression of the range may differ for each variant in V. The totality of all values of all these specific ranges we denote by R.

Finally, the behaviors differ among themselves according to the subject. "A white subject" is also a variable here, in that the name of any member of the given white population can replace the generic term; the behaviors are of individual subjects. A given population of white subjects, such as the Brazilian, is indicated here by P.

DESIGN OF THE EMPIRICAL RESEARCH

An ideal, complete research project designed according to the facets A, B, C, V, R, and P would consist of observing a value in the range R for each subject in population P on each variant in V for each profile in AB. (The facet C is already implicitly taken care of by defining V to be the union of V_1 and V_2 , so we need not refer to C directly here.) Using the convenient terminology and notation of set theory, the complete research would map PABV into R:

$$(1) \text{ PABV} \longrightarrow \text{R.}$$

The empirical observations would be a subset of the Cartesian product PABVR.

It may be suggested that any coherent social theory referring to empirical research can be expressed as a mapping such as (1). The number and nature of the facets may vary widely from those of our present example, both for the domain and for the range,

but the idea of a mapping seems indispensable. Lack of theoretical clarity as to the specifications of the facets of the mapping may be the situation that often impedes the connection between abstract theory and empirical work.

From the empirical results implied by formula (1), it is possible to calculate the statistical relations among all the variants of behavior for all profiles of AB for the given population. But, along with Bastide and van den Berghe, we may not be interested in such a detailed statistical analysis. These authors have assigned, for each of their four subuniverses, a *numerical value* to each subject, based on his behavior with respect to all the variants of that subuniverse. In essence, they have derived from mapping (1) a new mapping:

$$(2) \text{ PABC} \longrightarrow \text{S,}$$

where S is the continuum of real numbers.

Mapping (2) is of course more complete than that carried out by Bastide and van den Berghe: it implies the assignment of eight real numbers, or scores, to each subject in P—one score for each profile in ABC. Since the Brazilian study included only four profiles from ABC, it derived only four scores per subject.

EMPIRICAL SCALABILITY AND CORRELATION COEFFICIENTS

No essential loss of information occurs in moving from mapping (1) to mapping (2) should each subuniverse of variants prove empirically to be perfectly scalable for P. From the scale scores on a subuniverse one could then reproduce the behavior on the variants. For example, if s were the score of subject p for subuniverse $a_1 b_1 c_1$, then from s and knowledge of the empirical scale structure one could deduce each value of R for p on each profile of $a_1 b_1 V_1$. In the case of quasi-scalability, one could parsimoniously deduce at least the frequencies of joint occurrence of values from R for each pair of profiles from $a_1 b_1 V_1$ for population P.

Bastide and van den Berghe apparently carried out no tests of scalability for their data. It seems plausible, however, that each of their subuniverses should be at least quasi-scalable. For purposes of our present dis-

cussion, let us accept the hypothesis that numerical scores may be sufficient for studying the empirical interrelationships among the subuniverses.

While empirical scalability as such merely assures a meaningful rank order of the subjects in *P* for each subuniverse, these ranks may often be monotonely transformed into scores, all of which have *linear regressions* on each other.⁸ In such a case, the resulting scores are generally uniquely determined (when expressed in standard form: with zero means and unit variances). More importantly, the relations between subuniverses can then be expressed by ordinary product-moment correlation coefficients.

Simple additive scoring—as that used in the Brazilian study—of quasi-scalable items must closely approximate the underlying rank order if the number of items is large. It may also approximate the regression-linearizing scores quite well. Let us, then, accept the empirical correlation coefficients of the Brazilian study as a correct picture of the relationships among the subuniverses considered.

PREDICTED AND ACTUAL STRUCTURE OF THE
EMPIRICAL CORRELATION MATRIX:
THE CONTIGUITY HYPOTHESIS

Given mapping (2), the empirical product-moment correlations between the eight sub-

only of the semantics of universe ABC, but we do propose to predict a *pattern* or structure for the *relative* sizes of the statistical coefficients from purely semantic considerations.

Since the Brazilian data allow us to check our prediction only with respect to the inter-correlations of scores on subuniverses I, II, III, and IV, we concentrate first on these. Our prediction is based on the following proposition:

Contiguity Hypothesis. Subuniverses closer to each other in the semantic scale of their definitions will also be closer statistically.

Statistical closeness is measured approximately (but not always exactly, as we shall indicate below) by correlation coefficients. According to the contiguity hypothesis, generally Stereotype should correlate more highly with Norm than with Hypothetical Interaction, and almost certainly more than with Personal Interaction: $r_{I\ II} > r_{I\ III} > r_{I\ IV}$. Similarly, we should have approximately $r_{II\ III} > r_{II\ IV}$, $r_{III\ II} > r_{III\ I}$, and $r_{IV\ III} > r_{IV\ II} > r_{IV\ I}$. These exhaust the predictions that are directly possible, or where differing degrees of contiguity are defined by semantic rank order considerations for the subuniverses.

The actual correlation matrix reported by the authors of the Brazilian study is equivalent to the presentation in Table 2. The

TABLE 2. EMPIRICAL INTERCORRELATIONS OF SCORES ON THE FOUR SUBUNIVERSES

Subuniverse	I Stereotype	II Norm	III Hypothetical Interaction	IV Personal Interaction
I. Stereotype	—	.60	.37	.25
II. Norm	.60	—	.68	.51
III. Hypothetical Interaction	.37	.68	—	.49
IV. Personal Interaction	.25	.51	.49	—

universes of ABC for population *P* may be calculated. Our analysis above of the semantic structure of ABC provides a social-theoretical basis for predicting the structure of this empirical correlation matrix. One cannot presume to predict the exact size of each correlation coefficient from knowledge

structure of this correlation table is virtually as predicted. The largest correlations tend to be adjacent to the main diagonal, corresponding to the semantically contiguous subuniverses—and they taper off to the northeast and southwest corners of the table, where semantic differences increase.

An apparent slight exception to this structure is that $r_{IV\ III}$ (= .49) does not quite exceed $r_{IV\ II}$ (= .51), despite the fact that

⁸ Louis Guttman, "Metricing Rank Ordered and Unordered Data for a Linear Factor Analysis," *Sankhya* (in press).

semantically III lies between II and IV. This need be no actual contradiction of the contiguity hypothesis, as should become evident in the explicit discussion of statistical distances below. Sampling error, of course, might also be offered as a tentative explanation for the apparent aberration, but we prefer to ignore such "outs" for the purpose of the exposition of our theory. The selection of the 580 subjects used from the Brazilian P is described as "neither random nor proportional," even this need not affect our structural theory. The relative patterning of correlations need not change even though the absolute sizes of the coefficients might. Idiosyncracies of sampling and biased selections of subjects often can violently affect arithmetic means and other averages, as well as variances and other measures of dispersion. Correlation coefficients are often harder to destroy or to build up artificially, and the possible attenuation or disattenuation is even less likely to alter the *pattern* of correlations, since such effects usually influence correlation coefficients by constants of proportionality.

A SYSTEM OF ELEMENTARY STATISTICAL COMPONENTS

A correlation matrix which reveals a simple gradient similar to the one given above has been termed a *simplex*.⁹ It is possible to make more precise statistical hypotheses, based on the semantic structures of the subuniverses, in order to arrive at a more precise specification of the gradient among the empirical correlation coefficients. One way of doing this is to hypothesize a system of elementary statistical components—or of common-factors, in the language of factor analysis—that gives rise to the observed coefficients, associating the statistical components, however, directly with the semantic components.

Let s_{1p} be the score observed for subject p on subuniverse I. Since this score was obtained by mapping $a_1 b_1 c_1$ into S, let us assume that it is a function of a series of subscores for person p, one for each of the components of profile $a_1 b_1 c_1$, say scores denoted as a_{1p} , b_{1p} , and c_{1p} . Verbally, a_{1p} denotes a

generalized score for subject p on Belief concerning behavior with Negroes; the score is more positive when subject p's beliefs are more positive, and the score is more negative in the converse case. Similarly, b_{1p} is a generalized score on Subject's Group, being more positive as the subject's reference to his group's behavior vis-a-vis Negroes is more favorable. And c_{1p} is a generalized score on Comparative Behavior, increasing as p ascribes better traits to Negroes.

Interaction scores are also possible for pairs of components, such as $a_1 b_1$, $a_1 c_1$, and $b_1 c_1$. These we shall ignore here for convenience (moreover, apparently they are not essential to the present numerical example). The highest order interaction, assigned to the total profile $a_1 b_1 c_1$, may be considered the *unique factor score* for p, in the language of factor analysis, and we shall denote it by z_{1p} . This score is specific to subuniverse I as a whole.

Going in the reverse direction, we may consider a most general score, for the universe as a whole, and denote it by u_p . This implies an underlying generalized behavior, ranging from unfavorable to favorable toward Negroes and unmodified by any of the facets of ABC.

Thus far the metrics of S are arbitrary. To focus on correlation coefficients is to focus implicitly on the scores as in standard form: with zero means and unit variances. Let us seek more "natural" metrics, or constants of proportionality, w_I , w_{II} , w_{III} , and w_{IV} , which will enable us to write the unattenuated and "natural" scores

$$t_{1p} = w_{I1p} s_{1p} - z_{1p} \quad t_{IIp} = w_{II1p} s_{IIp} - z_{IIp} \\ t_{IIIp} = w_{III1p} s_{IIIp} - z_{IIIp} \quad t_{IVp} = w_{IV1p} s_{IVp} - z_{IVp},$$

which have the following statistical structure assumed to be associated with the facet design:

- (3) $t_{1p} = u_p + a_{1p} + b_{1p} + c_{1p}$
- (4) $t_{IIp} = u_p + a_{1p} + b_{1p} + c_{2p}$
- (5) $t_{IIIp} = u_p + a_{1p} + b_{2p} + c_{2p}$
- (6) $t_{IVp} = u_p + a_{2p} + b_{2p} + c_{2p}$.

For details of such a transformation of scores s to scores t see the reference in footnote 9.

THE SIMPLEX FACTOR PATTERN

As a consequence of the scalable semantic structure underlying the various scores in

⁹ Guttman, "A New Approach to Factor Analysis: the Radex," *op. cit.*

TABLE 3. THE EMPIRICAL PARTIAL CORRELATION COEFFICIENTS FOR THE FOUR SUBUNIVERSES

Subuniverse	I Stereotype	II Norm	III Hypothetical Interaction	IV Personal Interaction
I. Stereotype	—	.51	-.05	-.07
II. Norm	.51	—	.52	.27
III. Hypothetical Interaction	-.05	.52	—	.22
IV. Personal Interaction	-.07	.27	.22	—

equations (3)–(6), each t differs from its predecessor by a difference of scores on only a single facet. Let the differences within facet scores be respectively denoted by:

$$a_p = a_{2p} - a_{1p} \quad \beta_p = b_{2p} - b_{1p} \quad \text{and} \quad \gamma_p = c_{2p} - c_{1p}.$$

Then subtracting equation (3) from (4), (4) from (5), and (5) from (6) yield, respectively,

$$t_{IIp} - t_{Ip} = \gamma_p$$

$$t_{IIIp} - t_{IIp} = \beta_p$$

$$t_{IVp} - t_{IIIp} = a_p.$$

Alternatively, we can write a new factor pattern equivalent to (3)–(6):

$$(7) \quad t_{Ip} = t_{Ip}$$

$$(8) \quad t_{IIp} = t_{Ip} + \gamma_p$$

$$(9) \quad t_{IIIp} = t_{Ip} + \gamma_p + \beta_p$$

$$(10) \quad t_{IVp} = t_{Ip} + \gamma_p + \beta_p + a_p.$$

Introducing now the assumption of zero covariances among α , β , and γ makes equations (7)–(10) exactly equivalent to those of a general type of simplex analyzed in detail elsewhere,¹⁰ and predicting the empirical structure of Table 2. Equations (7)–(10) show how each variable differs from its predecessor by an additional factor, so that the correlation between any two variables should decrease in general as the variables depart from each other in the given rank order.

FURTHER EMPIRICAL VERIFICATION: THE PARTIAL AND MULTIPLE CORRELATIONS

Not only have the zero-order correlations of Table 2 their approximate pattern predicted from the simplex structure of equations (7)–(10), but the partial correlations have a similarly predicted pattern, and indeed may often show it more sharply.¹¹ The

¹⁰ Louis Guttman, "A Generalized Simplex for Factor Analysis," *Psychometrika*, 20 (September, 1955), pp. 173–192.

¹¹ *Ibid.*

empirical partial correlation between each pair of behaviors, holding the others constant, is given in Table 3.

The gradients among the coefficients in Table 3 are much sharper than in Table 2, showing even more unmistakably the rank order among the subuniverses. Subuniverse IV once more does not conform to the pattern in the simplest way, but again does not negate the ordering. The partial correlation between subuniverses III and IV would have behaved more nicely were it larger than its given value of .22.

Further confirmation of the order is provided by the multiple-correlation coefficients of each observed variable on the remaining ones. These are listed in Table 4, where they are also compared with the highest zero-order correlations of the respective variables with any other in Table 2.

The first subuniverse in the rank order, Stereotype, is no more predictable from all three remaining subuniverses than it is from merely its immediate neighbor, Norm. This should be expected from equations (3)–(6), or alternatively from (7)–(10), which hypothesize that the non-neighbors of Stereotype introduce further factors not contained in and hence not predictive of Stereotype. On the other hand, Norm does gain in predictability by considering more than one neighbor, but essentially by considering its two immediate neighbors in the rank order, I and III. Partial correlation coefficients

TABLE 4. MULTIPLE AND MAXIMAL ZERO-ORDER CORRELATIONS FOR THE FOUR SUBUNIVERSES

Subuniverse	Multiple Correlation Coefficient	Maximal Zero-Order Correlation Coefficient
I. Stereotype	.60	.60
II. Norm	.80	.68
III. Hypothetical Interaction	.70	.68
IV. Personal Interaction	.55	.51

being proportional to multiple regression weights, the coefficients in row II of Table 3 imply that most of the multiple predictability of Norm is dependent on subuniverses I and III.

Hypothetical Interaction does not appreciably gain by using a multiple rather than a simple regression. This is also explained by Table 3, where row III shows that only Norm receives a substantial multiple regression weight, the other neighbor, Personal Interaction, failing to give as large a coefficient as might be expected from the contiguity hypothesis. The result is that subuniverse III is about as predictable from one neighbor alone as from all three remaining subuniverses.

The same holds for the last subuniverse in the rank order. It has but one immediate neighbor, of course, and is about as predictable from it ($r=.49$) as from the multiple regression. By the slight quirk already noted, the correlation of IV with II is a bit larger ($r=.51$) and both III and II receive about equal weight in the multiple regression for IV, according to row IV of Table 3. This does not destroy the established order, of course, but it would have been neater to have II less interchangeable with III as a predictor of IV, in light of the fact that II is clearly between I and III for all the other regressions.

STATISTICAL RELATIONS FOR THE REMAINING SUBUNIVERSSES

Had we empirical data for the complete matrix of correlations among all eight subuniverses, the validity of our structural theory as thus far developed could be checked further. Equations similar to (3)-(6) and (7)-(10) can be written for each of the other possible semantic scales based on all of ABC. Each observed subuniverse score can be expressed as a sum of t_i and some combination of α , β , and γ , apart from its unique factor score. The hypothesis of zero correlations among α , β , and γ would then enable us to calculate the distances between any two subuniverses, once the statistical distances are calculated for the subuniverses of the Brazilian study. The empirical results could then be compared with the calculated results, and thus verify the adequacy of the structural theory.

Failure of the results to check out could imply either or both of the following possibilities: the statistical structure deduced from the semantic structure was not appropriate; the semantic structure was faulty or incomplete.

A more complete analysis would seek to estimate the scores on each of the common-factors in equations (3)-(6). As these equations stand, there are more common factors (namely, seven) than observed variables. This situation is remedied when all eight subuniverse scores are considered simultaneously, for no further common-factors are introduced thereby. The technicalities involved go far beyond simplex analysis, and take us into the more general field of statistical facet analysis.¹²

SUMMARY

Three facets (subject's behavior, referent, and referent's intergroup behavior) of two elements each suffice to distinguish eight important subuniverses of intergroup behavior. Differences within each subuniverse are defined by two further facets, the variants and the ranges of the respective behaviors. Statistical distances between the subuniverses were predicted to have a certain order structure related to the facet design of the semantic definition of the subuniverses. This statistical structure was verified empirically for the four subuniverses studied in Brazil by Bastide and van den Bergh. An approximate simplex order holds among these latter four subuniverses, so that the maximum predictability of each is virtually attainable from its immediate neighbor or neighbors alone. To increase the predictability would require enriching the facet design, or placing these behaviors in a larger context.

APPENDIX: RELATION TO COMMON-FACTOR ANALYSIS

For readers interested in the relationship between the approach presented above and a type of common-factor analysis with which they may be more familiar, the following remarks may be helpful.

(a) Our factor structure is not obtained by a "blind" analysis, but is hypothesized directly

¹² This point is sketched in Guttman, "What Lies Ahead for Factor Analysis?" *op. cit.*

from a facet design based on social psychological consideration. "Blind" analyses are now known to be essentially incapable of revealing ordered structures, such as the simplex, even when the order is strikingly apparent to the eye and is the most parsimonious way of viewing the data.

(b) At least two different rotations of axes are meaningful for our case: equations (3)-(6) and equations (7)-(10). In each case, our factors are "named" in advance. A third rotation—to principal axes—may also prove meaningful eventually, when the appropriate psychology is worked out, as discussed in the references of footnotes 4 and 10. But our structural conclusions depend on none of these: the additivity of distance functions is all we need and this requires no particular location of reference axes.

(c) Even after eliminating unique factors, we have left as many common-factors as observed variables, as in equations (7)-(10), or even more common-factors than observed variables,

as in equation (3)-(6). New light is cast by radex theory on the communality problem of factor analysis: the assumption that parsimony is equivalent to smallness of number of common-factors has been shown to be unfounded.¹³

(d) Using variances as distance functions, instead of standard deviations, implies using a non-Euclidean metric for the variables. The variables lie in one dimension—along a straight line, and in the semantic scale order—when our non-Euclidean metric is used; but they define an n -dimensional space—where n is the number of common-factors—when the Euclidean metric is used.

The orthogonality conditions between α , β , and γ lay no restrictions on their covariances with t_i in pattern (7)-(10); t_i may be oblique or orthogonal to each of these latter factors, without affecting the additivity of distances.

¹³ *Ibid.*; and Louis Guttman, "To What Extent Can Communalities Reduce Rank?" *Psychometrika*, 23 (December, 1958), pp. 297-308.

SOME STATISTICAL PROBLEMS IN RESEARCH DESIGN *

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Several statistical problems in the design of research are discussed: (1) The use of statistical tests and the search for causation in survey research are examined; for this we suggest separating four classes of variables: explanatory, controlled, confounded, and randomized. (2) The relative advantages of experiments, surveys, and other investigations are shown to derive respectively from better control, representation, and measurement. (3) Finally, three common misuses of statistical tests are examined: "hunting with a shot-gun for significant differences," confusing statistical significance with substantive importance, and overemphasis on the primitive level of merely finding differences.

STATISTICAL inference is an important aspect of scientific inference. The statistical consultant spends much of his time in the borderland between statistics and the other aspects, philosophical and substantive, of the scientific search for explanation. This marginal life is rich both in direct experience and in discussions of fundamentals; these have stimulated my concern with the problems treated here.

I intend to touch on several problems dealing with the interplay of statistics with the more general problems of scientific in-

ference. We can spare elaborate introductions because these problems are well known. Why then discuss them here at all? We do so because, first, they are problems about which there is a great deal of misunderstanding, evident in current research; and, second, they are *statistical* problems on which there is broad agreement among research statisticians—and on which these statisticians generally disagree with much in the current practice of research scientists.¹

¹ Cf. R. A. Fisher, *The Design of Experiments*, London: Oliver and Boyd, 6th edition, 1953, pp. 1-2: "The statistician cannot evade the responsibility for understanding the processes he applies or recommends. My immediate point is that the questions involved can be disassociated from all that is strictly technical in the statistician's craft, and *when so detached*, are questions only of the right use of human reasoning powers, with which all intel-

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Several problems will be considered briefly, hence incompletely. The aim of this paper is not a profound analysis, but a clear elementary treatment of several related problems. The footnotes contain references to more thorough treatments. Moreover, these are not *all* the problems in this area, nor even necessarily the most important ones; the reader may find that his favorite, his most annoying problem, has been omitted. The problems selected are a group with a common core, they arise frequently, yet they are widely misunderstood.

STATISTICAL TESTS OF SURVEY DATA

That correlation does not prove causation is hardly news. Perhaps the wittiest statements on this point are in George Bernard Shaw's preface to *The Doctor's Dilemma*, in the sections on "Statistical Illusions," "The Surprises of Attention and Neglect," "Stealing Credit from Civilization," and "Biometrika." (These attack, alas, the practice of vaccination.) The excellent introductory textbook by Yule and Kendall² deals in three separate chapters with the problems of advancing from correlation to causation. Searching for causal factors among survey data is an old, useful sport; and the attempts to separate true explanatory variables from extraneous and "spurious" correlations have taxed scientists since antiquity and will undoubtedly continue to do so. Neyman and Simon³ show that beyond common sense, there are some technical skills involved in tracking down spurious correlations. Econometricians and geneticists have developed great interest and skill in the prob-

ligent people, who hope to be intelligible, are equally concerned, and on which the statistician, as such, speaks with no special authority. The statistician cannot excuse himself from the duty of getting his head clear on the principles of scientific inference, but equally no other thinking man can avoid a like obligation."

² G. Undy Yule and M. G. Kendall, *An Introduction to the Theory of Statistics*, London: Griffin, 11th edition, 1937, Chapters 4, 15, and 16.

³ Jerzy Neyman, *Lectures and Conferences on Mathematical Statistics and Probability*, Washington, D. C.: Graduate School of Department of Agriculture, 1952, pp. 143-154. Herbert A. Simon, "Spurious Correlation: A Causal Interpretation," *Journal of the American Statistical Association*, 49 (September, 1954), pp. 467-479; also in his *Models of Man*, New York: Wiley, 1956.

lems of separating the explanatory variables.⁴

The researcher designates the explanatory variables on the basis of substantive scientific theories. He recognizes the evidence of other *sources of variation* and he needs to separate these from the explanatory variables. Sorting all sources of variation into four classes seems to me a useful simplification. Furthermore, no confusion need result from talking about sorting and treating "variables," instead of "sources of variation."

I. The *explanatory* variables, sometimes called the "experimental" variables, are the objects of the research. They are the variables among which the researcher wishes to find and to measure some specified relationships. They include both the "dependent" and the "independent" variables, that is, the "predictand" and "predictor" variables.⁵ With respect to the aims of the research all other variables, of which there are three classes, are extraneous.

II. There are extraneous variables which are *controlled*. The control may be exercised in either or both the selection and the estimation procedures.

⁴ See the excellent and readable article, Herman Wold, "Causal Inference from Observational Data," *Journal of the Royal Statistical Society (A)*, 119 (Part 1, January, 1956), pp. 28-61. Also the two-part technical article, M. G. Kendall, "Regression, Structure and Functional Relationship," *Biometrika*, 38 (June, 1951), pp. 12-25; and 39 (June, 1952), pp. 96-108. The interesting methods of "path coefficients" in genetics have been developed by Wright for inferring causal factors from regression coefficients. See, in Oscar Kempthorne *et al.*, *Statistics and Mathematics in Biology*, Ames, Iowa: The Iowa State College Press, 1954; Sewall Wright, "The Interpretation of Multi-Variate Systems," Chapter 2; and John W. Tukey, "Causation, Regression and Path Analysis," Chapter 3. Also C. C. Li, "The Concept of Path Coefficient and Its Impact on Population Genetics," *Biometrics*, 12 (June, 1956), pp. 190-209. I do not know whether these methods can be of wide service in current social science research in the presence of numerous factors, of large unexplained variances, and of doubtful directions of causation.

⁵ Kendall points out that these latter terms are preferable. See his paper cited in footnote 4, and M. G. Kendall and W. R. Buckland, *A Dictionary of Statistical Terms*, Prepared for the International Statistical Institute with assistance of UNESCO, London: Oliver and Boyd, 1957. I have also tried to follow in IV below his distinction of "variate" for random variables from "variables" for the usual (nonrandom) variable.

III. There may exist extraneous uncontrolled variables which are *confounded* with the Class I variables.

IV. There are extraneous uncontrolled variables which are treated as *randomized* errors. In "ideal" experiments (discussed below) they are actually randomized; in surveys and investigations they are only assumed to be randomized. Randomization may be regarded as a substitute for experimental control or as a form of control.

The aim of efficient design both in experiments and in surveys is to place as many of the extraneous variables as is feasible into the second class. The aim of randomization in experiments is to place all of the third class into the fourth class; in the "ideal" experiment there are no variables in the third class. And it is the aim of controls of various kinds in surveys to separate variables of the third class from those of the first class; these controls may involve the use of repeated cross-tabulations, regression, standardization, matching of units, and so on.

The function of statistical "tests of significance" is to test the effects found among the Class I variables against the effects of the variables of Class IV. An "ideal" experiment here denotes an experiment for which this can be done through randomization without any possible confusion with Class III variables. (The difficulties of reaching this "ideal" are discussed below.) In survey results, Class III variables are confounded with those of Class I; the statistical tests actually contrast the effects of the random variables of Class IV against the explanatory variables of Class I confounded with unknown effects of Class III variables. In both the ideal experiment and in surveys the statistical tests serve to separate the effects of the random errors of Class IV from the effects of other variables. These, in surveys, are a mixture of explanatory and confounded variables; their separation poses severe problems for logic and for scientific methods; statistics is only one of the tools in this endeavor. The scientist must make many decisions as to which variables are extraneous to his objectives, which should and can be controlled, and what methods of control he should use. He must decide where and how to introduce statistical tests of hypotheses into the analysis.

As a simple example, suppose that from a probability sample survey of adults of the United States we find that the level of political interest is higher in urban than in rural areas. A test of significance will show whether or not the difference in the "levels" is large enough, compared with the sampling error of the difference, to be considered "significant." Better still, the confidence interval of the difference will disclose the limits within which we can expect the "true" population value of the difference to lie.⁶ If families had been sent to urban and rural areas respectively, after the randomization of a true experiment, then the sampling error would measure the effects of Class IV variables against the effects of urban *versus* rural residence on political interest; the difference in levels beyond sampling errors could be ascribed (with specified probability) to the effects of urban *versus* rural residence.

Actually, however, residences are not assigned at random. Hence, in survey results, Class III variables may account for some of the difference. If the test of significance rejects the null hypothesis of no difference, several hypotheses remain in addition to that of a simple relationship between urban *versus* rural residence and political interest. Could differences in income, in occupation, or in family life cycle account for the difference in the levels? The analyst may try to remove (for example, through cross-tabulation, regression, standardization) the effects due to such variables, which are extraneous to his expressed interest; then he computes the difference, between the urban and rural residents, of the levels of interest now free of several confounding variables. This can be followed by a proper test of significance—or, preferably, by some other form of statistical inference, such as a statement of confidence intervals.

Of course, other variables of Class III may remain to confound the measured relationship between residence and political interest.

⁶ The sampling error measures the chance fluctuation in the difference of levels due to the sampling operations. The computation of the sampling error must take proper account of the actual sample design, and not blindly follow the standard simple random formulas. See Leslie Kish, "Confidence Intervals for Complex Samples," *American Sociological Review*, 22 (April, 1957), pp. 154-165.

The separation of Class I from Class III variables should be determined in accord with the nature of the hypothesis with which the researcher is concerned; finding and measuring the effects of confounding variables of Class III tax the ingenuity of research scientists. But this separation is beyond the functions and capacities of the statistical tests, the tests of null hypotheses. Their function is not explanation; they cannot point to causation. Their function is to ask: "Is there anything in the data that needs explaining?"—and to answer this question with a certain probability.

Agreement on these ideas can eliminate certain confusion, exemplified by Selvin in a recent article:

Statistical tests are unsatisfactory in non-experimental research for two fundamental reasons: it is almost impossible to design studies that meet the conditions for using the tests, and the situations in which the tests are employed make it difficult to draw correct inferences. The basic difficulty in design is that sociologists are unable to randomize their uncontrolled variables, so that the difference between "experimental" and "control" groups (or their analogs in nonexperimental situations) are a mixture of the effects of the variable being studied and the uncontrolled variables or correlated biases. Since there is no way of knowing, in general, the sizes of these correlated biases and their directions, there is no point in asking for the probability that the observed differences could have been produced by random errors. The place for significance tests is after all relevant correlated biases have been controlled. . . . In design and in interpretation, in principle and in practice, tests of statistical significance are inapplicable in nonexperimental research.⁷

Now it is true that in survey results the explanatory variables of Class I are confounded with variables of Class III; but it does not follow that tests of significance should not be used to separate the random variables of Class IV. Insofar as the effects found "are a mixture of the effects of the variable being studied and the uncontrolled

variables;" insofar as "there is no way of knowing, in general, the sizes" and directions of these uncontrolled variables, Selvin's logic and advice should lead not only to the rejection of statistical tests; it should lead one to refrain altogether from using survey results for the purposes of finding explanatory variables. In this sense, not only tests of significance but any comparisons, any scientific inquiry based on surveys, any scientific inquiry other than an "ideal" experiment, is "inapplicable." That advice is most unrealistic. In the (unlikely) event of its being followed, it would sterilize social research—and other nonexperimental research as well.

Actually, much research—in the social, biological, and physical sciences—must be based on nonexperimental methods. In such cases the rejection of the null hypothesis leads to several alternate hypotheses that may explain the discovered relationships. It is the duty of scientists to search, with painstaking effort and with ingenuity, for bases on which to decide among these hypotheses.

As for Selvin's advice to refrain from making tests of significance until "after all relevant" uncontrolled variables have been controlled—this seems rather far-fetched to scientists engaged in empirical work who consider themselves lucky if they can explain 25 or 50 per cent of the total variance. The control of all relevant variables is a goal seldom even approached in practice. To postpone to that distant goal all statistical tests illustrates that often "the perfect is the enemy of the good."⁸

⁸ Selvin performs a service in pointing to several common mistakes: (a) The mechanical use of "significance tests" can lead to false conclusions. (b) Statistical "significance" should not be confused with substantive importance. (c) The probability levels of the common statistical tests are not appropriate to the practice of "hunting" for a few differences among a mass of results. However, Selvin gives poor advice on what to do about these mistakes; particularly when, in his central thesis, he reiterates that "tests of significance are inapplicable in nonexperimental research," and that "the tests are applicable only when all relevant variables have been controlled." I hope that the benefits of his warnings outweigh the damages of his confusion.

I noticed three misleading references in the article. (a) In the paper which Selvin appears to use as supporting him, Wold (*op. cit.*, p. 39) specifically disagrees with Selvin's central thesis, stat-

⁷ Hanan C. Selvin, "A Critique of Tests of Significance in Survey Research," *American Sociological Review*, 22 (October, 1957), p. 527. In a criticism of this article, McGinnis shows that the separation of explanatory from extraneous variables depends on the type of hypothesis at which the research is aimed. Robert McGinnis, "Randomization and Inference in Sociological Research," *American Sociological Review*, 23 (August, 1958), pp. 408-414.

EXPERIMENTS, SURVEYS, AND OTHER INVESTIGATIONS

Until now, the theory of sample surveys has been developed chiefly to provide descriptive statistics—especially estimates of means, proportions, and totals. On the other hand, experimental designs have been used primarily to find explanatory variables in the analytical search of data. In many fields, however, including the social sciences, survey data must be used frequently as the analytical tools in the search for explanatory variables. Furthermore, in some research situations, neither experiments nor sample surveys are practical, and other investigations are utilized.

By "experiments" I mean here "ideal" experiments in which all the extraneous variables have been randomized. By "surveys" (or "sample surveys"), I mean probability samples in which all members of a defined population have a known positive probability of selection into the sample. By "investigations" (or "other investigations"), I mean the collection of data—perhaps with care, and even with considerable control—without either the randomization of experiments or the probability sampling of surveys. The differences among experiments, surveys, and investigations are not the consequences of statistical techniques; they result from different methods for introducing the variables and for selecting the population elements (subjects). These problems are ably treated in recent articles by Wold and Campbell.⁹

ing that "The need for testing the statistical inference is no less than when dealing with experimental data, but with observational data other approaches come to the foreground." (b) In discussing problems caused by complex sample designs, Selvin writes that "Such errors are easy enough to discover and remedy" (p. 520), referring to Kish (*op. cit.*). On the contrary, my article pointed out the seriousness of the problem and the difficulties in dealing with it. (c) "Correlated biases" is a poor term for the confounded uncontrolled variables and it is not true that the term is so used in literature. Specifically, the reference to Cochran is misleading, since he is dealing there only with errors of measurement which may be correlated with the "true" value. See William G. Cochran, *Sampling Techniques*, New York: Wiley, 1953, p. 305.

⁹ Wold, *op. cit.*; Donald T. Campbell, "Factors Relevant to the Validity of Experiments in Social Settings," *Psychological Bulletin*, 54 (July, 1957), pp. 297-312.

In considering the larger ends of any scientific research, only part of the total means required for inference can be brought under objective and firm control; another part must be left to more or less vague and subjective—however skillful—judgment. The scientist seeks to maximize the first part, and thus to minimize the second. In assessing the ends, the costs, and the feasible means, he makes a strategic choice of methods. He is faced with the three basic problems of scientific research: measurement, representation, and control. We ignore here the important but vast problems of measurement and deal with representation and control.

Experiments are strong on control through randomization; but they are weak on representation (and sometimes on the "naturalism" of measurement). Surveys are strong on representation, but they are often weak on control. Investigations are weak on control and often on representation; their use is due frequently to convenience or low cost and sometimes to the need for measurements in "natural settings."

Experiments have three chief advantages: (1) Through randomization of extraneous variables the confounding variables (Class III) are eliminated. (2) Control over the introduction and variation of the "predictor" variables clarifies the *direction* of causation from "predictor" to "predictand" variables. In contrast, in the correlations of many surveys this direction is not clear—for example, between some behaviors and correlated attitudes. (3) The modern design of experiments allows for great flexibility, efficiency, and powerful statistical manipulation, whereas the analytical use of survey data presents special statistical problems.¹⁰

The advantages of the experimental method are so well known that we need not dwell on them here. It is the scientific method *par excellence*—when feasible. In many situations experiments are not feasible and this is often the case in the social sciences; but it is a mistake to use this situation to separate the social from the physical and biological sciences. Such situations also occur frequently in the physical sciences (in meteorology, astronomy, geology), the biological sciences, medicine, and elsewhere.

¹⁰ Kish, *op. cit.*

The experimental method also has some shortcomings. First, it is often difficult to choose the "control" variables so as to exclude *all* the confounding extraneous variables; that is, it may be difficult or impossible to design an "ideal" experiment. Consider the following examples: The problem of finding a proper control for testing the effects of the Salk polio vaccine led to the use of an adequate "placebo." The Hawthorne experiment demonstrated that the design of a proposed "treatment *versus* control" may turn out to be largely a test of *any* treatment *versus* lack of treatment.¹¹ Many of the initial successes reported about mental therapy, which later turn into vain hopes, may be due to the hopeful effects of *any* new treatment in contrast with the background of neglect. Shaw, in "The Surprises of Attention and Neglect" writes: "Not until attention has been effectually substituted for neglect as a general rule, will the statistics begin to show the merits of the particular methods of attention adopted."

There is an old joke about the man who drank too much on four different occasions, respectively, of scotch and soda, bourbon and soda, rum and soda, and wine and soda. Because he suffered painful effects on all four occasions, he ascribed, with scientific logic, the common effect to the common cause: "I'll never touch soda again!" Now, to a man (say, from Outer Space) ignorant of the common alcoholic content of the four "treatments" and of the relative physiological effects of alcohol and carbonated water, the subject is not fit for joking, but for further scientific investigation.

Thus, the advantages of experiments over surveys in permitting better control are only

relative, not absolute.¹² The design of proper experimental controls is not automatic; it is an art requiring scientific knowledge, foresight in planning the experiment, and hindsight in interpreting the results. Nevertheless, the distinction in control between experiments and surveys is real and considerable; and to emphasize this distinction we refer here to "ideal" experiments in which the control of the random variables is complete.

Second, it is generally difficult to design experiments so as to represent a specified important population. In fact, the questions of sampling, of making the experimental results representative of a specified population, have been largely ignored in experimental design until recently. Both in theory and in practice, experimental research has often neglected the basic truth that causal systems, the distributions of relations—like the distributions of characteristics—exists only within specified universes. The distributions of relationships, as of characteristics, exist only within the framework of specific populations. Probability distributions, like all mathematical models, are abstract systems; their application to the physical world must include the specification of the populations. For example, it is generally accepted that the statement of a value for mean income has meaning only with reference to a specified population; but this is not generally and clearly recognized in the case of regression of assets on income and occupation. Similarly, the *statistical* inferences derived from the experimental testing of several treatments are restricted to the population(s) included in the experimental design.¹³ The clarification of the population sampling aspects of experiments is now being tackled vigorously by Wilk and Kempthorne and by Cornfield and Tukey.¹⁴

¹¹ F. J. Roethlisberger and W. J. Dickson, *Management and the Worker*, Cambridge: Harvard University Press, 1939. Troubles with experimental controls misled even the great Pavlov into believing *temporarily* that he had proof of the inheritance of an acquired ability to learn: "In an informal statement made at the time of the Thirteenth International Physiological Congress, Boston, August, 1929, Pavlov explained that in checking up these experiments it was found that the apparent improvement in the ability to learn, on the part of successive generations of mice, was really due to an improvement in the ability to teach, on the part of the experimenter." From B. G. Greenberg, *The Story of Evolution*, New York: Garden City, 1929, p. 327.

¹² Jerome Cornfield, "Statistical Relationships and Proof in Medicine," *American Statistician*, 8 (December, 1954), pp. 19-21.

¹³ McGinnis, *op. cit.*, p. 412, points out that usually "it is not true that one can uncover 'general' relationships by examining some arbitrarily selected population. . . . There is no such thing as a completely general relationship which is independent of population, time, and space. The extent to which a relationship is constant among different populations is an empirical question which can be resolved only by examining different populations at different times in different places."

¹⁴ Martin B. Wilk and Oscar Kempthorne, "Some

Third, for many research aims, especially in the social sciences, contriving the desired "natural setting" for the measurements is not feasible in experimental design. Hence, what social experiments give sometimes are clear answers to questions the meanings of which are vague. That is, the artificially contrived experimental variables may have but a tenuous relationship to the variables the researcher would like to investigate.

The second and third weaknesses of experiments point to the advantages of surveys. Not only do probability samples permit clear statistical inferences to defined populations, but the measurements can often be made in the "natural settings" of actual populations. Thus in practical research situations the experimental method, like the survey method, has its distinct problems and drawbacks as well as its advantages. In practice one generally cannot solve simultaneously all of the problems of measurement, representation, and control; rather, one must choose and compromise. In any specific situation one method may be better or more practical than the other; but there is no over-all superiority in all situations for either method. Understanding the advantages and weaknesses of both methods should lead to better choices.

In social research, in preference to both surveys and experiments, frequently some design of controlled investigation is chosen—for reasons of cost or of feasibility or to preserve the "natural setting" of the measurements. Ingenious adaptations of experimental designs have been contrived for these controlled investigations. The statistical framework and analysis of experimental designs are used, but not the randomization of true experiments. These designs are aimed to provide flexibility, efficiency, and, especially, some control over the extraneous variables. They have often been used to improve con-

siderably research with controlled investigations.

These designs are sometimes called "natural experiments." For the sake of clarity, however, it is important to keep clear the distinctions among the methods and to reserve the word "experiment" for designs in which the uncontrolled variables are randomized. This principle is stated clearly by Fisher,¹⁵ and is accepted often in scientific research. Confusion is caused by the use of terms like "ex post facto experiments" to describe surveys or designs of controlled investigations. Sample surveys and controlled investigations have their own justifications, their own virtues; they are not just second-class experiments. I deplore the borrowing of the prestige word "experiment," when it cloaks the use of other methods.

Experiments, surveys, and investigations can all be improved by efforts to overcome their weaknesses. Because the chief weakness of surveys is their low degree of control, researchers should be alert to the collection and use of auxiliary information as controls against confounding variables. They also should take greater advantage of changes introduced into their world by measuring the effects of such changes. They should utilize more often efficient and useful statistics instead of making tabular presentation their only tool.

On the other hand, experiments and controlled investigations can often be improved by efforts to specify their populations more clearly and to make the results more representative of the population. Often more should be done to broaden the area of inference to more important populations. Thus, in many situations the deliberate attempts of the researcher to make his sample more "homogeneous" are misplaced; and if common sense will not dispel the error, reading Fisher may.¹⁶ When he understands this,

¹⁵ Fisher, *op. cit.*, pp. 17-20. "Controlled investigation" may not be the best term for these designs. "Controlled observations" might do, but "observation" has more fundamental meanings.

¹⁶ *Ibid.*, pp. 99-100. Fisher says: "We have seen that the factorial arrangement possesses two advantages over experiments involving only single factors: (i) Greater efficiency, in that these factors are evaluated with the same precision by means of only a quarter of the number of observations that would otherwise be necessary; and (ii) Greater

Aspects of the Analysis of Factorial Experiment in a Completely Randomized Design," *Annals of Mathematical Statistics*, 27 (December, 1956), pp. 950-985; and "Fixed, Mixed and Random Models," *Journal of the American Statistical Association*, 50 (December, 1955), pp. 1144-1167. Jerome Cornfield and John W. Tukey, "Average Values of Mean Squares in Factorials," *Annals of Mathematical Statistics*, 27 (December, 1956), pp. 907-949.

the researcher can view the population base of his research in terms of efficiency—in terms of costs and variances. He can often avoid basing his research on a comparison of one sampling unit for each "treatment." If he cannot obtain a proper sample of the entire population, frequently he can secure, say, four units for each treatment, or a score for each.¹⁷

Suppose, for example, that thorough research on one city and one rural county, discloses higher levels of political interest in the former. It is presumptuous (although common practice) to present this result as evidence that urban people in *general* show a higher level. (Unfortunately, I am not beating a dead horse; this nag is pawing daily in the garden of social science.) However, very likely there is a great deal of variation in political interest among different cities, as well as among rural counties; the results of the research will depend heavily on which city and which county the researcher picked as "typical." The research would have a broader base if a city and a rural county

comprehensiveness in that, in addition to the 4 effects of single factors, their 11 possible interactions are evaluated. There is a third advantage which, while less obvious than the former two, has an important bearing upon the utility of the experimental results in their practical application. This is that any conclusion, such as that it is advantageous to increase the quantity of a given ingredient, has a wider inductive basis when inferred from an experiment in which the quantities of other ingredients have been varied, than it would have from any amount of experimentation, in which these had been kept strictly constant. The exact standardisation of experimental conditions, which is often thoughtlessly advocated as a panacea, always carries with it the real disadvantage that a highly standardized experiment supplies direct information only in respect of the narrow range of conditions achieved by standardisation. Standardisation, therefore, weakens rather than strengthens our ground for inferring a like result, when, as is invariably the case in practice, these conditions are somewhat varied."

¹⁷ For simplicity the following illustration is a simple contrast between two values of the "explanatory" variable, but the point is more general; and this aspect is similar whether for true experiments or controlled observations. Incidentally, it is poor strategy to "solve" the problem of representation by obtaining a good sample, or complete census, of some small or artificial population. A poor sample of the United States or of Chicago usually has more over-all value than the best sample of freshman English classes at X University.

would have been chosen in each of, say, four different situations—as different as possible (as to region, income, industry, for example); or better still in twenty different situations. A further improvement would result if the stratification and selection of sampling units followed a scientific sample design.

Using more sampling units and spreading them over the breadth of variation in the population has several advantages. First, some measure of the variability of the observed effect may be obtained. From a probability sample, statistical inference to the population can be made. Second, the base of the inference is broadened, as the effect is observed over a variety of situations. Beyond this lies the combination of results from researches over several distinct cultures and periods. Finally, with proper design, the effects of several potentially confounding factors can be tested.

These points are brought out by Keyfitz in an excellent example of controlled investigation (which also uses sampling effectively): "Census enumeration data were used to answer for French farm families of the Province of Quebec the question: Are farm families smaller near cities than far from cities, other things being equal? The sample of 1,056 families was arranged in a 2⁶ factorial design which not only controlled 15 extraneous variables (income, education, etc.) but incidentally measured the effect of 5 of these on family size. A significant effect of distance from cities was found, from which is inferred a geographical dimension for the currents of social change."¹⁸ The mean numbers of children per family were found to be 9.5 near and 10.8 far from cities; the difference of 1.3 children has a standard error of 0.28.

SOME MISUSES OF STATISTICAL TESTS

Of the many kinds of current misuses this discussion is confined to a few of the most common. There is irony in the circumstance that these are committed usually by the more statistically inclined investigators; they are avoided in research pre-

¹⁸ Nathan Keyfitz, "A Factorial Arrangement of Comparisons of Family Size," *American Journal of Sociology*, 53 (March, 1953), p. 470.

sented in terms of qualitative statements or of simple descriptions.

First, there is "hunting with a shot-gun" for significant differences. Statistical tests are designed for distinguishing results at a pre-determined level of improbability (say at $P = .05$) under a specified null hypothesis of random events. A rigorous theory for dealing with individual experiments has been developed by Fisher, the Pearsons, Neyman, Wold, and others. However, the researcher often faces more complicated situations, especially in the analysis of survey results; he is often searching for interesting relationships among a vast number of data. The keen-eyed researcher hunting through the results of one thousand random tosses of perfect coins would discover and display about fifty "significant" results (at the $P = .05$ level).¹⁹ Perhaps the problem has become more acute now that high-speed computers allow hundreds of significance tests to be made. There is no easy answer to this problem. We must be constantly aware of the nature of tests of null hypotheses in searching survey data for interesting results. After finding a result improbable under the null hypothesis the researcher must not accept blindly the hypothesis of "significance" due to a presumed cause. Among the several alternative hypotheses is that of having discovered an improbable random event through sheer diligence. Remedy can be found sometimes by a reformulation of the statistical aims of the research so as to fit the available tests. Unfortunately, the classic statistical

tests give clear answers only to some simple decision problems; often these bear but faint resemblance to the complex problems faced by the scientist. In response to these needs the mathematical statisticians are beginning to provide some new statistical tests. Among the most useful are the new "multiple comparison" and "multiple range" tests of Tukey, Duncan, Scheffé,²⁰ and others. With a greater variety of statistical statements available, it will become easier to choose one without doing great violence either to them or to the research aims.

Second, statistical "significance" is often confused with and substituted for substantive significance. There are instances of research results presented in terms of probability values of "statistical significance" alone, without noting the magnitude and importance of the relationships found. These attempts to use the probability levels of significance tests as measures of the strengths of relationships are very common and very mistaken. The function of statistical tests is merely to answer: Is the variation great enough for us to place some confidence in the result; or, contrarily, may the latter be merely a happenstance of the specific sample on which the test was made? This question is interesting, but it is surely *secondary*, auxiliary, to the main question: Does the result show a relationship which is of substantive interest because of its nature and its magnitude? Better still: Is the result consistent with an assumed relationship of substantive interest?

The results of statistical "tests of significance" are functions not only of the magnitude of the relationships studied but also of the numbers of sampling units used (and the efficiency of design). In small samples significant, that is, meaningful, results may fail to appear "statistically significant." But if the sample is large enough the most insignificant relationships will appear "statistically significant."

Significance should stand for meaning and refer to substantive matter. The statistical

¹⁹ William H. Sewell, "Infant Training and the Personality of the Child," *American Journal of Sociology*, 53 (September, 1952), pp. 150-159. Sewell points to an interesting example: "On the basis of the results of this study, the general null hypothesis that the personality adjustments and traits of children who have undergone varying training experiences do not differ significantly cannot be rejected. Of the 460 chi square tests, only 18 were significant at or beyond the 5 per cent level. Of these, 11 were in the expected direction and 7 were in the opposite direction from that expected on the basis of psychoanalytic writings. . . . Certainly, the results of this study cast serious doubts on the validity of the psychoanalytic claims regarding the importance of the infant disciplines and on the efficacy of prescriptions based on them" (pp. 158-159). Note that by chance alone one would expect 23 "significant" differences at the 5 per cent level. A "hunter" would report either the 11 or the 18 and not the hundreds of "misses."

²⁰ John W. Tukey, "Comparing Individual Means in the Analysis of Variance," *Biometrics*, 5 (June, 1949), pp. 99-114; David B. Duncan, "Multiple Range and Multiple F Tests," *Biometrics*, 11 (March, 1955), pp. 1-42; Henry Scheffé, "A Method for Judging All Contrasts in the Analysis of Variance," *Biometrika*, 40 (June, 1953), pp. 87-104.

tests merely answer the question: Is there a big enough relationship here which *needs* explanation (and is not merely chance fluctuation)? The word *significance* should be attached to another question, a substantive question: Is there a relationship here *worth* explaining (because it is important and meaningful)? As a remedial step I would recommend that statisticians discard the phrase "test of significance," perhaps in favor of the somewhat longer but proper phrase "test against the null hypothesis" or the abbreviation "TANH."

Yates, after praising Fisher's classic *Statistical Methods*, makes the following observations on the use of "tests of significance":

Second, and more important, it has caused scientific research workers to pay undue attention to the results of the tests of significance they perform on their data, particularly data derived from experiments, and too little to the estimates of the magnitude of the effects they are investigating.

Nevertheless the occasions, even in research work, in which quantitative data are collected solely with the object of proving or disproving a given hypothesis are relatively rare. Usually quantitative estimates and fiducial limits are required. Tests of significance are preliminary or ancillary.

The emphasis on tests of significance, and the consideration of the results of each experiment in isolation, have had the unfortunate consequence that scientific workers have often regarded the execution of a test of significance on an experiment as the ultimate objective. Results are significant or not significant and this is the end of it.²¹

For presenting research results statistical estimation is more frequently appropriate than tests of significance. The estimates should be provided with some measure of sampling variability. For this purpose confidence intervals are used most widely. In large samples, statements of the standard errors provide useful guides to action. These problems need further development by theoretical statisticians.²²

The responsibility for the current fashions should be shared by the authors of statistical

textbooks and ultimately by the mathematical statisticians. As Tukey puts it:

Statistical methods should be tailored to the real needs of the user. In a number of cases, statisticians have led themselves astray by choosing a problem which they could solve exactly but which was far from the needs of their clients. . . . The broadest class of such cases comes from the choice of significance procedures rather than confidence procedures. It is often much easier to be "exact" about significance procedures than about confidence procedures. By considering only the most null "null hypothesis" many inconvenient possibilities can be avoided.²³

Third, the tests of null hypotheses of zero differences, of no relationships, are frequently weak, perhaps trivial statements of the researcher's aims. In place of the test of zero difference (the nuldest of null hypotheses), the researcher should often substitute, say, a test for a difference of a specific size based on some specified model. Better still, in many cases, instead of the tests of significance it would be more to the point to measure the magnitudes of the relationships, attaching proper statements of their sampling variation. The magnitudes of relationships cannot be measured in terms of levels of significance; they can be measured in terms of the difference of two means, or of the proportion of the total variance "explained," of coefficients of correlations and of regressions, of measures of association, and so on. These views are shared by many, perhaps most, consulting statisticians—although they have not published full statements of their philosophy. Savage expresses himself forcefully: "Null hypotheses of no difference are usually known to be false before the data are collected; when they are, their rejection or acceptance simply reflects the size of the sample and the power of the test, and is not a contribution to science."²⁴

Too much of social research is planned and presented in terms of the mere existence of some relationship, such as: individuals

²¹ Frank Yates, "The Influence of *Statistical Methods for Research Workers* on the Development of the Science of Statistics," *Journal of the American Statistical Association*, 46 (March, 1951), pp. 32-33.

²² D. R. Cox, "Some Problems Connected with Statistical Inference," *Annals of Mathematical Statistics*, 29 (June, 1958), pp. 357-372.

²³ John W. Tukey, "Unsolved Problems of Experimental Statistics," *Journal of the American Statistical Association*, 49 (December, 1954), p. 710. See also D. R. Cox, *op. cit.*, and David B. Duncan, *op. cit.*

²⁴ Richard J. Savage, "Nonparametric Statistics," *Journal of the American Statistical Association*, 52 (September, 1957), pp. 332-333.

high on variate x are also high on variate y . The *exploratory* stage of research may be well served by statements of this order. But these statements are relatively weak and can serve *only* in the primitive stages of research. Contrary to a common misconception, the more advanced stages of research should be phrased in terms of the quantitative aspects of the relationships. Again, to quote Tukey:

There are normal sequences of growth in immediate ends. One natural sequence of immediate ends follows the sequence: (1) Description, (2) Significance statements, (3) Estimation, (4) Confidence statement, (5) Evaluation. . . . There are, of course, other normal sequences of immediate ends, leading mainly through various decision procedures, which are appropriate to development research and to operations research, just as the sequence we have just discussed is appropriate to basic research.²⁵

At one extreme, then, we may find that the contrast between two "treatments" of a labor force results in a difference in productivity of 5 per cent. This difference may appear "statistically significant" in a sample of, say, 1000 cases. It may also mean a difference of millions of dollars to the company. However, it "explains" only about one per

cent of the total variance in productivity. At the other extreme is the far-away land of completely determinate behavior, where every action and attitude is explainable, with nothing left to chance for explanation.

The aims of most basic research in the social sciences, it seems to me, should be somewhere between the two extremes; but too much of it is presented at the first extreme, at the primitive level. This is a matter of over-all strategy for an entire area of any science. It is difficult to make this judgment off-hand regarding any specific piece of research of this kind: the status of research throughout the entire area should be considered. But the superabundance of research aimed at this primitive level seems to imply that the over-all strategy of research errs in this respect. The construction of scientific theories to cover broader fields—the persistent aim of science—is based on the synthesis of the separate research results in those fields. A coherent synthesis cannot be forged from a collection of relationships of unknown strengths and magnitudes. The necessary conditions for a synthesis include an *evaluation* of the results available in the field, a coherent interrelating of the *magnitudes* found in those results, and the construction of models based on those magnitudes.

²⁵ Tukey, *op. cit.*, pp. 712-713.

THE GEOMETRIC INTERPRETATION OF AGREEMENT

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This paper refers to a prior article by the author in which the statistical idea of "agreement" as contrasted with correlation was developed and shown to be measured appropriately by the intraclass correlation coefficient or a simple modification of it. Section I of this paper is entirely expository, and simplifies the original algebraic argument for the non-mathematical reader by discussing some properties of agreement with reference to a series of simple graphs. Section II, by adding a geometric counterpart to the original algebraic definition of agreement, spells out further implications of agreement as a concept.

SOME time ago I published a paper on "The Statistical Measurement of Agreement,"¹ in which I developed the idea of agreement as contrasted with correlation from first principles, and showed that the intraclass correlation coefficient or a simple

modification of it was an appropriate measure of agreement. The argument of the paper was algebraic and somewhat compact.

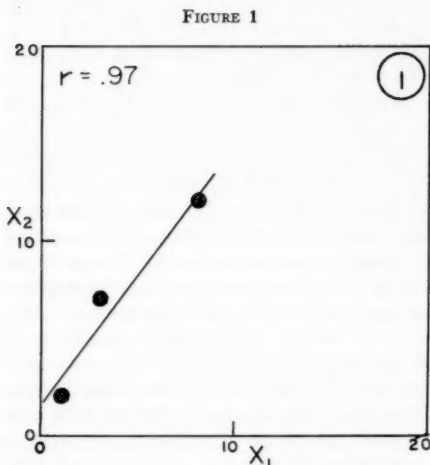
The original characterization of agreement was as follows: "Agreement requires that paired values be identical, while correlation requires only that paired values be linked by a linear relationship, or, if one defines correlation more broadly, that the paired values

¹ W. S. Robinson, *American Sociological Review*, 22 (February, 1957), pp. 17-25.

be linked according to some mathematical function. Perfect agreement has but one form, $X_1 = X_2$, whereas correlation may variously be written $X_1 = a + bX_2$, $X_1 = a + bX_2 + cX_2^2$, $X_1 = \log X_2$, etc. Thus agreement is a special case of correlation, since two variables that agree must be correlated, but variables which are correlated do not necessarily agree. The research significance of the distinction is that agreement must be measured against the model $X_1 = X_2$, while correlation may be measured against any functional relationship as model. . . .²

I

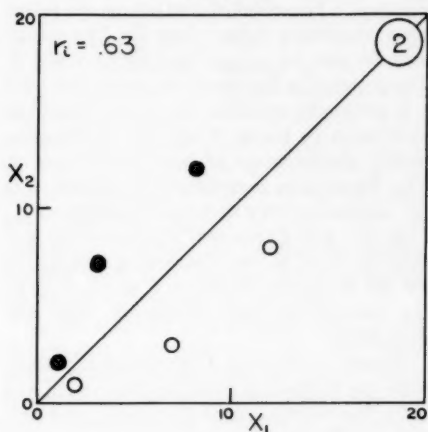
To turn now to a graphic exposition of agreement, Figure 1 is a Pearsonian (interclass) scatter diagram showing the correlation between two variables X_1 and X_2 for three paired values of these variables.



The coordinates of the points in Figure 1 are (1,2), (3,7), and (8,12), the first number referring to the value of X_1 and the second to the value of X_2 . The line in Figure 1 is the ordinary Pearsonian regression line for estimating values of X_2 from values of X_1 with least-squares errors. There is of course a second regression line, for estimating values of X_1 from values of X_2 . Because of the very high correlation in Figure 1, however, this second line is almost identical with the first,

² *Op. cit.*, p. 19.

FIGURE 2



and it is impossible to represent it on a small diagram.

Figure 2 shows the *intraclass* scatter diagram for the three observations of Figure 1. To get the intraclass scatter diagram, one plots the three observations on X_1 and X_2 just as in Figure 1, and then reverses the values of X_1 and X_2 and plots these three additional "observations" also, shown in Figure 2 as circles instead of dots. That is, one plots not only the points (1,2), (3,7), and (8,12), but also the points, referred to the original axes, (2,1), (7,3), and (12,8).³

The line in Figure 2 is not a regression line. One might instead call it the *line of agreement*. It is not computed from the observations, but is merely drawn on the scatter diagram so as to connect points for which $X_1 = X_2$. It is thus the line on which the observations of Figure 1 would fall if for each pair of values of X_1 and X_2 the values were in perfect agreement.

The three additional "observations" in the intraclass scatter diagram (Figure 2) are symmetrically displaced from the three original observations (solid dots) of the Pearsonian scatter diagram across the line of agreement, normal (or at right angles) to it, and at equal distances from it on the other side.

Now the intraclass correlation for the three original observations in Figure 1 is by definition the Pearsonian correlation between the

³ *Op. cit.*, p. 20.

six points of the intraclass scatter diagram of Figure 2.⁴ The effect of computing the intraclass correlation rather than the Pearsonian may be seen by comparing Figures 1 and 2. The correlation for the three points in Figure 1 is obviously considerably greater than the correlation in Figure 2 because of the symmetric displacement of points in Figure 2. The Pearsonian correlation for Figure 1 is .97, indicating very high predictability of X_2 from X_1 , and *vice versa*. The intraclass correlation, that is, the Pearsonian correlation for the six points of Figure 2, is .63, indicating considerably less agreement than predictability.

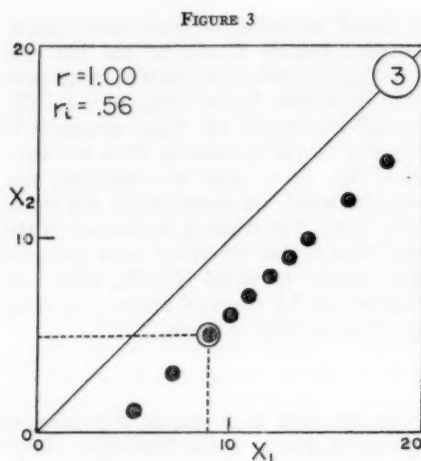
Figures 3 through 7 systematically illustrate the distinction between Pearsonian and intraclass correlation or agreement, and provide graphic illustrations of the equation for the relation between the two, namely,

$$r_1 = \frac{[(s_1^2 + s_2^2) - (s_1 - s_2)^2] r - (\bar{X}_1 - \bar{X}_2)^2 / 2}{(s_1^2 + s_2^2) + (\bar{X}_1 - \bar{X}_2)^2 / 2}, \quad (1)$$

"where \bar{X}_1 and \bar{X}_2 denote the means of X_1 and X_2 , s_1 and s_2 the standard deviations, and r the Pearsonian correlation between X_1 and X_2 ."⁵ (r_1 is of course the intraclass correlation coefficient). The figures are standardized, with scales of from zero to 20 for both X_1 and X_2 .

In Figure 3, in which the Pearsonian correlation is unity, the intraclass correlation is .56. This difference in values shows the effect of a systematic difference in origin (or level) between the two variables. While X_1 and X_2 are perfectly correlated in a Pearsonian sense, the value of X_1 is four units greater than the corresponding value of X_2 for each observation, and the small value of the intraclass correlation coefficient reflects this fact: that X_1 and X_2 assign different values to the same observation. X_1 and X_2 , of course, need not be perfectly correlated for this to occur. All that is required is that on the average X_1 values be greater than X_2 values, or *vice versa*. (This is the case in Figure 1.)

For example, consider the encircled observation in Figure 3, and assume that X_1 and X_2 are two different measures of socioeconomic status which have been applied to the individual represented by that observation. According to the scale of X_1 , as shown



by the vertical dotted line, this individual has a socioeconomic status score of 9. However, according to the scale of X_2 , as shown by the horizontal dotted line, the same individual has a socioeconomic status score of 5. The values do not agree.

Whenever there is such systematic disagreement in level or size of scores as assessed by two variables, the means of X_1 and X_2 will differ, that is, the value of $(\bar{X}_1 - \bar{X}_2)^2 / 2$ in Equation (1) will not be zero. Since the standard deviations of X_1 and X_2 in Figure 3 are the same, the difference between Pearsonian and intraclass correlations shows the pure effect of difference in origin between the two variables. That is, $(s_1 - s_2)$ is zero in Equation (1), and the difference between Pearsonian and intraclass correlations is attributable solely to the difference between the means.

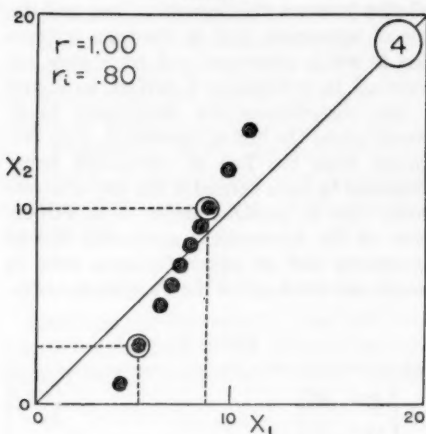
Incidentally, one can gain a rough idea of the difference between the Pearsonian and intraclass correlations by mentally projecting the observed values of Figure 3 normally across the line of agreement and at equal distances from it. One then sees two sets of points running parallel to the line of agreement and at equal distances from it (the intraclass scatter diagram), and the Pearsonian correlation for these two sets of points (the intraclass correlation) is by inspection much smaller than the Pearsonian correlation for the original set of points.

Figure 4 shows the effect of a difference in scale (or unit) between X_1 and X_2 . In Figure

⁴ *Loc. cit.*

⁵ *Op. cit.*, p. 21, Equation (8).

FIGURE 4



4, while the means (or levels) of X_1 and X_2 are the same, the standard deviation (or unit) of X_1 is only half the standard deviation of X_2 . X_1 and X_2 are still perfectly correlated in a Pearsonian sense, but the intraclass correlation coefficient is .80. X_1 in Figure 4 assesses a difference between two observations as being only half as great as the difference would be if measured in terms of X_2 , and the difference between Pearsonian and intraclass correlations reflects the failure of X_1 and X_2 to agree in this respect. Of course X_1 and X_2 do not have to be perfectly correlated for this to occur. All that is required is that the swarm of points have a slope different from unity.

To see what a difference in scale or unit or standard deviation means in terms of the difference between Pearsonian and intraclass correlations, consider the *two* encircled observations in Figure 4, and for concreteness assume again that X_1 and X_2 are two different measures of socioeconomic status which have been applied to the two individuals represented by the encircled observations. The values of X_1 for the two encircled observations are 8.75 and 5.25, as indicated by the vertical dotted lines. The difference between the X_1 values of the two observations is thus $8.75 - 5.25$, or 3.50.

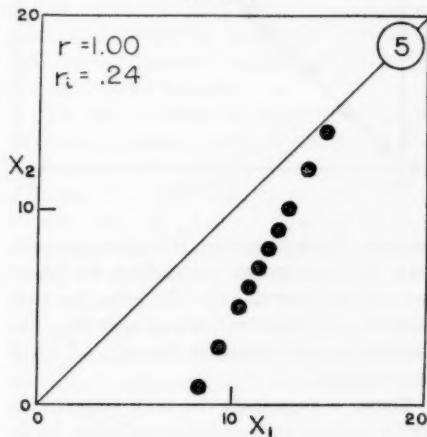
However, if one measures the socioeconomic status values of the two observations in terms of X_2 , as indicated by the horizontal dotted lines in Figure 4, one finds that the values for the two encircled observations are

10 and 3. The difference between the socioeconomic status scores of the two individuals as measured by X_2 is thus $10 - 3$, or 7, twice the difference when assessed by X_1 .

Moreover, the difference between Pearsonian and intraclass correlations in Figure 4 shows the pure effect of difference in scale or unit between X_1 and X_2 because for Figure 4 the means of X_1 and X_2 are equal, and thus $(\bar{X}_1 - \bar{X}_2)^2/2$ is zero. Thus the only thing in Equation (1) that can account for the difference between Pearsonian and intraclass correlations is $(s_1 - s_2)$.

Again, one can gain a rough idea of how the difference in unit will affect the Pearsonian correlation by mentally projecting the observed values of Figure 4 normally (at right angles) across the line of agreement and at equal distances from it. It is apparent that this projection will result in two crossing sets of points on straight lines, and it is also apparent that the Pearsonian correlation

FIGURE 5



for these two sets of points (the intraclass correlation) will be smaller than the Pearsonian correlation for the original set of points. It is also apparent from the same mental inspection that the Pearsonian correlation for the two crossing sets of points will be greater than the Pearsonian correlation for the imagined intraclass scatter diagram of Figure 3, and thus that the intraclass correlation for Figure 3 will be lower than that for Figure 4.

Figure 5 shows the joint effect of the

difference in origin (or level) of Figure 3 and the difference in scale (or unit) of Figure 4. While the Pearsonian correlation for Figure 5 is still unity, the intraclass correlation or agreement has dropped to a mere .24. As in Figures 3 and 4, one can for Figure 5 get a rough idea of how agreement will penalize Pearsonian correlation by mentally projecting the observed points in Figure 5 across the line of agreement. The result will be two sets of observations falling upon straight lines and forming a V with its apex in the upper right corner of the scatter

Figure 7 is introduced to make clear the relation between the regression lines and the line of agreement, and to illustrate a situation in which agreement and correlation are identical, though neither is perfect. In Figure 7 the observations are distributed fairly evenly about the line of agreement, their distances from the line of agreement being measured by lines normal to the line of agreement, that is, at right angles to it. Projections of the observations across the line of agreement and at equal distances from it would not much affect the Pearsonian corre-

FIGURE 6

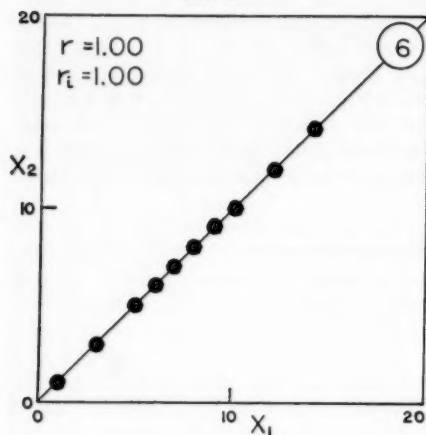
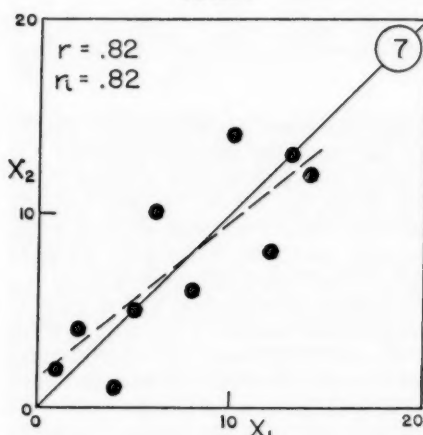


diagram. From inspection it is then apparent that the Pearsonian correlation for these two sets of observations (the intraclass correlation) will be very much smaller than the Pearsonian correlation for the original set of observations.

Figure 6 represents the only situation in which perfect Pearsonian correlation corresponds to perfect agreement—a situation in which all observations fall upon the line of agreement. In Figure 6, projection of the observations “across” the line of agreement does not displace them at all, and the Pearsonian and intraclass scatter diagrams are identical. It is plain that for Figure 6 the distributions of X_1 and X_2 are identical, and thus that their means are equal and their standard deviations are equal. Therefore, by virtue of Equation (1), the Pearsonian and intraclass correlations will be equal also.

FIGURE 7



lation: the Pearsonian and intraclass scatter diagrams would have about the same Pearsonian correlation. In fact, the Pearsonian and intraclass correlations for Figure 7 are identical and both equal to .82. The identity of the two coefficients follows from the fact that in Figure 7 the means of X_1 and X_2 are the same and the standard deviations of X_1 and X_2 are the same. Thus by virtue of Equation (1) the two coefficients must be identical.

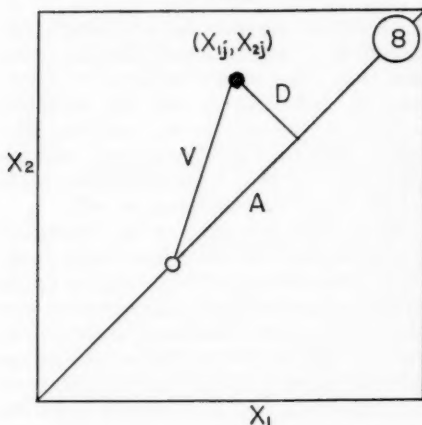
However, this does not mean that the line of agreement is a regression line. Only in the special case of Figure 6 are the two regression lines and the line of agreement coincident. The regression line for estimating values of X_2 from values of X_1 is shown in Figure 7 as a broken line, and it is evidently not coincident with the line of agreement. The regression lines and the line of agree-

ment are defined to express different things. Deviations from the regression line in Figure 7, for example, are measured vertically, that is, with reference to the scale of X_2 , while deviations from the line of agreement are measured normal (at right angles) to the line of agreement. While the regression lines and the line of agreement have interesting mathematical relationships, these are not treated here.

II

The foregoing discussion is concerned with graphic illustrations of the relation between correlation and agreement. It will now be

FIGURE 8



shown, however, that the original algebraic definition of "a fundamental measure of agreement"⁶ has a geometric interpretation which throws additional light on the meaning of agreement as a statistical concept.

Consider in Figure 8 a single point from a *Pearsonian* scatter diagram, chosen to illustrate the argument that follows. The point has coordinates X_{1j} and X_{2j} referred to the X_1 and X_2 axes, respectively. The subscript j merely indicates that the values of X_1 and X_2 are for the j -th pair of values of X_1 and X_2 .

The diagonal line in Figure 8 is the line of agreement, connecting points for which $X_1 = X_2$. The point on the line of agreement

indicated by **O** has X_1 and X_2 coordinates both equal to $(\bar{X}_1 + \bar{X}_2)/2$ and will be called the *mean of the intraclass system*. For our purpose it serves merely as a reference point on the line of agreement.⁷

Now consider a straight line drawn from the point **O** to the observed point which has coordinates X_{1j} and X_{2j} . For want of a better name, this line is called the *pair vector* for the observed point, that is, the vector representing the pair of values X_{1j} and X_{2j} . It is denoted by **V** (for vector) in Figure 8. The position of the observation, of course, is as well defined by the length and direction of the vector as it is by its X_1 and X_2 coordinates.

The line **D** in Figure 8 is a straight line drawn from the observed point (X_{1j}, X_{2j}) normal, or perpendicularly, to the line of agreement and intersecting it. The line **D** is termed the *disagreement component* of the point (X_{1j}, X_{2j}) or, more properly, of the pair vector **V**. It is so termed because its length is the distance from the observed point to the line of agreement, that is, a measure of the extent to which the values of X_{1j} and X_{2j} fail to agree.

The line **A**, falling on the line of agreement and extending from the point **O** to the intersection of **D** with the line of agreement, is named the *agreement component* of **V** or of the point (X_{1j}, X_{2j}) . It is apparent that the lines **V**, **D**, and **A** form a right triangle, **D** and **A** being the two components in the resolution of **V**.

It is easily shown that for the N observations in a *Pearsonian* scatter diagram the coefficient of agreement **A**⁸ can be simply ex-

⁷ In ordinary correlation theory, the mean of the *Pearsonian* (or interclass) system has coordinates \bar{X}_1 and \bar{X}_2 referred to the X_1 and X_2 axes, respectively. It is the point through which both regression lines pass and hence the point at which they intersect. For an intraclass scatter diagram, which incidentally Figure 8 is not, the point **O** is the mean of the intraclass system, and the point at which the two (meaningless) regression lines intersect. The relation between the mean of a *Pearsonian* system and an intraclass system is simple; if the mean of a *Pearsonian* system is projected normal (or at right angles) to the line of agreement, it intersects the line of agreement at the mean of the intraclass system.

⁸ Robinson, *op. cit.*, Equation (6).

⁶ *Op. cit.*, pp. 19-20, through Equation (6).

pressed in terms of the N lengths of V , D , and A for the N observations.⁹ In fact:

$$A = 1 - \Sigma D^2 / \Sigma V^2, \quad (2)$$

$$= 1 - \frac{\text{Mean square length of the } N \text{ disagreement components}}{\text{Mean square length of the } N \text{ pair vectors}}, \quad (3)$$

$$= \Sigma A^2 / \Sigma V^2, \quad (4)$$

$$= \frac{\text{Mean square length of the } N \text{ agreement components}}{\text{Mean square length of the } N \text{ pair vectors}}, \quad (5)$$

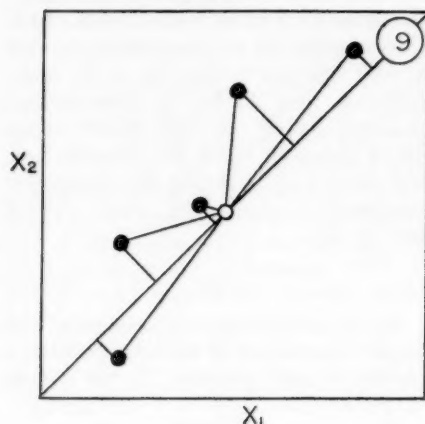
The original definition of "a fundamental measure of agreement"¹⁰ is

$$A = 1 - \frac{\Sigma (X_{1j} - \bar{X}_1)^2 + \Sigma (X_{2j} - \bar{X}_2)^2}{\Sigma (X_{1j} - \bar{X})^2 + \Sigma (X_{2j} - \bar{X})^2}, \quad (6)$$

where \bar{X}_j represents the mean of X_1 and X_2 for the j -th pair of values, and $\bar{X} = (\bar{X}_1 + \bar{X}_2)/2$, or the mean of all $2N$ observations taken together. The relation between the "fundamental measure of agreement" in Equation (6) and Equations (2) to (5) is very simple. The numerator of the fraction in Equation (6) is merely the sum of the squares of the N disagreement components in a Pearsonian scatter diagram, and the denominator is the sum of the squares of the N pair vectors. From this and the Pythagorean theorem follow Equations (2) to (5).

Figure 9 shows a Pearsonian scatter diagram in which the pair vectors and the disagreement and agreement components are all displayed. Each line drawn from the mean of the intraclass system O to an observation is a pair vector V . Each line running perpendicularly from an observation to the line of agreement is a disagreement component D . The agreement component A for each point is the distance along the line of agreement from the mean of the intraclass system O to the point at which the disagreement component intersects the line of agreement. In fact, one might compute the value of A approximately by measuring the lengths of the vectors and their components on the diagram and substituting their values into any one of Equations (2) to (5).

FIGURE 9



Agreement can now be very simply expressed in geometric terms. The discussion here is in terms of the coefficient of agreement A , but since A and the intraclass correlation coefficient for two variables differ only in that one varies from 0 to 1 and the other from -1 to $+1$, the explanation holds for the intraclass correlation as well.

When all of the points on the Pearsonian scatter diagram fall on the line of agreement, each agreement component is equal in length to the vector of which it is a component, and then by Equation (5) the agreement coefficient (as well as the intraclass correlation coefficient) is unity. If the observed points do not fall on the line of agreement, then the agreement components are shorter than the pair vectors, and by Equation (5) the agreement coefficient and the intraclass correlation are less than unity.

The pair vectors are so to speak "hinged" at the mean of the intraclass system O , and the intraclass correlation or coefficient of agreement is a measure of the tendency of the pair vectors to be collinear with the line of agreement, that is, to lie on the line of agreement.

Thus when the pair vectors are collinear with the line of agreement, the intraclass correlation and the agreement coefficient are unity. As the pair vectors swing out of collinearity with the line of agreement—as they make larger and larger angles with it—the agreement coefficient and the intraclass correlation diminish because the disagreement

⁹ A simple proof is available upon request to the author, Department of Anthropology and Sociology, University of California, Los Angeles 24, California.

¹⁰ Robinson, *op. cit.*, Equation (6).

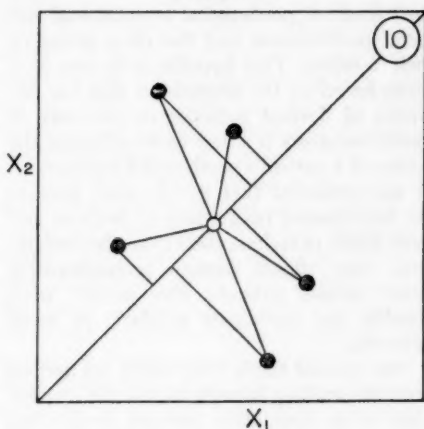
components account for more and more of the pair vectors and the agreement components account for less and less of the pair vectors.

If the pair vectors swing maximally away from the line of agreement they again become collinear, but the line on which they all fall is normal (at right angles) to the line of agreement. In this case each pair vector has but one component, the disagreement component; hence by Equation (5) the agreement coefficient is zero and the intraclass correlation takes on its minimum value of -1 . An approximation to this case is shown in Figure 10, in which the disagreement components are on the average much larger than the agreement components.

The preceding argument can be extended to the general case of k variables. Graphic representation of the geometry of agreement, however, is restricted to three variables at

the most, and the three-variable illustrations are too involved to be discussed here.

FIGURE 10



SOCIAL CLASS AND POSTHOSPITAL PERFORMANCE LEVELS *

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Harvard University

The departure point of this paper is the thesis that the differential commitment of relatives to the dominant values of the society has consequences for the posthospital fate of the mental patient. The major hypothesis tested concerns the relationship between patients' posthospital performance and the class status of their families. In addition, the relationships between performance levels and three other variables which presumably reflect degree of commitment to the dominant values—namely, class self-identification, religion, and ethnic background—are in the predicted direction. The documented relationships are part of a series based on the proposition that tolerance of deviant behavior on the part of family members is a key factor affecting the course of a patient's posthospital experience.

THE relationships of social class to such aspects of mental illness as etiology, epidemiology, diagnosis, acceptance of psychiatric services, and perceptions of illness have been the subject of considerable research.¹ Indeed, social class probably has

been employed more frequently than any other sociological variable in mental illness research, as well as in studies in other medical fields.² The demonstrated predictive power of this variable led us to include it in a systematic study of the posthospital experience of mental patients who remain in the community for extended periods after release from a hospital. Our major objective was to identify correlates of posthospital levels of occupational and social performance among these patients.

* This research is being undertaken by the Community Health Project, under the direction of Ozzie G. Simmons. The Project is sponsored by the Social Science Program at the Harvard School of Public Health, and is supported by a grant (M 1627) from the National Institute of Mental Health.

¹ Perhaps the best example is the research of Hollingshead, Redlich, and their associates. See August B. Hollingshead and Frederick C. Redlich, *Social Class and Mental Illness*, New York: Wiley, 1958.

² With respect to public health, for example, see Ozzie G. Simmons, *Social Status and Public Health* (Social Science Research Council Pamphlet 13), New York: Social Science Research Council, 1958.

The principal hypothesis examined here is that, among patients who remain in the community for a year or more after hospitalization, a direct relationship exists between their levels of posthospital occupational and social performance and the class status of their families. This hypothesis is one of a series based on the proposition that the tolerance of deviant behavior on the part of family members is a key factor affecting the course of a patient's posthospital experience.³ It was predicted that middle class families are less tolerant than others of deviants and more likely to exclude them from their midst; thus, they should contain proportionately fewer former patients who neither work steadily nor participate regularly in social activities.

The general thesis from which we derived both the working hypothesis and the proposition of an association between social class and tolerance of deviance is that the post-hospital fate of the patient is influenced by his family's commitment to the dominant values of the society. Families less committed to these values are more likely to tolerate deviant performance in occupational and other instrumental roles. Although variations in the degree of commitment to the dominant values are manifested in a variety of subcultural identifications and affiliations, social class is perhaps the most prominent and certainly the most frequently employed point of reference for the analysis of value differences in American society.

A number of conceptual formulations regarding the differential behavior and expectations of social classes, as well as various empirical studies, support this point of view.⁴

³ This proposition and the relationship of post-hospital performance to other variables have been reported by us in other papers. See "Mental Patients in the Community: Family Settings and Performance Levels," *American Sociological Review*, 23 (April, 1958), pp. 147-154; "Wives, Mothers, and the Posthospital Performance of Mental Patients," *Social Forces*, 37 (December, 1958), pp. 153-159; "Familial Expectations and Posthospital Performance of Mental Patients," *Human Relations* (in press); "The Social Integration of Former Mental Patients," *International Journal of Social Psychiatry* (in press).

⁴ For example, the studies reported in R. Bendix and S. H. Lipset, editors, *Class, Status and Power*, Glencoe, Ill.: Free Press, 1953, particularly sections 3 and 4.

The concern of families with "inadequate" occupational and social performance and consequent definition of the family member as a deviant require awareness of and readiness to act in the ways the dominant culture specifies as "appropriate." Although the power of the mass media has attenuated class differences in awareness of the dominant values of the society, differential exposure to the formal educational process (and to the variety of interpersonal situations for which considerable education is a prerequisite) continues to influence the internalization of values and readiness to act in accordance with them. Moreover, and perhaps most important, the primacy accorded work and other instrumental roles in the value hierarchy of middle class persons severely limits their tolerance of deviant performance in these roles.

In addition, the preoccupation with conformity to the prevailing standards that is more common among middle class persons with respect to the entire gamut of ideological imperatives—including, of course, work and social participation—also restricts their tolerance of deviant behavior. Accompanying these expectations of conformity is an elaborate and specific punitive system for dealing with the member whose behavior deviates from accepted norms. This strong emphasis upon conformity in the middle class has related consequences on the personality level, for example, the tendency to delay gratifications and control impulses.⁵ These observations have been integrated in analyses of the socio-economic aspiration pattern and are considered in a number of general commentaries on American society.⁶

Finally, although perhaps a less direct point, the ability of middle class persons to deal with complex, symbolic processes may also be related to the less frequent retention in these families of patients with low levels of performance. By virtue of the middle class emphasis upon and practice of interpersonal manipulation, these families can more readily mobilize and exploit the outside community so as to facilitate the re-

⁵ Cf. Louis Schneider and Sverre Lysgaard, "The Deferred Gratification Pattern: A Preliminary Study," *American Sociological Review*, 18 (April, 1953), pp. 142-149.

⁶ E.g., Robin M. Williams, Jr., *American Society*, New York: Knopf, 1955.

turn of the patient to the hospital. Moreover, the pronounced verbal ability of middle class persons is useful in effecting the rehospitalization of a family member in the face of the frequent reluctance of hospital personnel to readmit the former patient.

No attempt was made to test directly any of these points. Rather, these considerations led to the development of the hypothesis of a direct relationship between the class status of a patient's family and his levels of social and occupational performance. A number of other variables, in addition to social class, were included in the study to amplify the hypothesized relationship and to provide support for the general thesis of a linkage between tolerance of deviance and differential commitment to the dominant values. In addition to measures of objective class status, we have analyzed the relationship between performance levels and three other variables generally held to reflect differential commitment to the dominant values: religion, ethnic background, and class self-identification. We attempted to elicit directly expressions of values in one area, namely, socio-economic aspirations. These data are also included in the analysis.

THE STUDY GROUP

The female informants interviewed were all relatives, predominantly wives and mothers, of male patients who succeeded in remaining in the community since their latest release from a mental hospital sometime between November, 1954 and December, 1955.⁷ Every male patient was included in the potential study group who had the following characteristics: between 20 and 60 years of age, white, native born, living in the Boston area at the time of release, hospitalized more than 45 days prior to release, not physically handicapped to the extent of being unemployable, not addicted to narcotics, and not primarily hospitalized for acute alcoholism. By diagnosis, all were psychotics with non-organic disorders, the majority schizophrenic. Patients selected were last hospitalized in one of 13 mental hospitals in the Boston area. Of the 209 interviews attempted, 182 (88 per cent) were completed.

⁷ The study group is more fully described in "Mental Patients in the Community: Family Settings and Performance Levels," *op. cit.*

SOCIAL CLASS AND PERFORMANCE LEVELS

Performance level, as employed here, is a combined rating of patients' steadiness of posthospital employment and extent of participation in social activities. Patients who are rated high had worked full time since hospitalization and had participated regularly, and as frequently as other members of their families, in formal and informal social activities.⁸ Breadwinner's income, rent, and Warner's I.S.C. were selected as measures of social class. Each of these three indexes is significantly associated with the other two. Table 1 shows the relationships of performance levels to the social class variables, and the point biserial correlation coefficients (r_{pbs}) for these relationships. Although reported as dichotomies, the relationships remain stable when more finite categories are employed in ranking families on the three class measures.

There is also a direct relationship between performance levels of patients and the class self-identification of the informants (see Table 2). Indeed, the class self-perceptions of family members appear to be more closely associated with the performance level of the patient than are the objective status characteristics. In Table 3, the families are divided into three groups on the basis of their I.S.C. and self-identification responses. The largest proportion of high level patients is found in families rated middle class both objectively and subjectively. Compared with this group, families rated objectively as lower class have a slightly lower proportion of patients with high performance levels, even if the family member perceived herself as middle class. But the greatest contrast is between these two groups and families who are rated objectively as lower class and whose

⁸ These scales are more fully described in *Ibid.* Social participation was rated relative to the participation of other family members in order to compensate for class differences in informal and formal social activities. The findings reported here also apply when either work performance or social participation is employed by itself as a criterion of posthospital performance, and also hold when a "social integration scale" is employed as the dependent variable. The latter scale was constructed from the performance measures and a rating on psychiatric symptoms manifested by the patients; cf. "The Social Integration of Former Mental Patients," *op. cit.*

TABLE 1. PERFORMANCE LEVELS AND STATUS CHARACTERISTICS

Performance Level	Breadwinner's Income		I.S.C. Score		Rent	
	Under \$3900 per year	\$3900 and over per year	50 and over	Under 50	\$60 and under per month	Over \$60 per month
1 (high)	19.0%	48.2%	28.3%	47.7%	28.2%	43.3%
2	28.6	30.6	30.4	31.8	29.1	31.7
3	17.9	7.1	13.8	11.4	13.6	13.3
4	34.5	14.1	27.5	9.1	29.1	11.7
N=	84	85 ^a	138	44	117	60 ^b
r_{pb}	.35		.22		.21	

^a 13 cases not included because of no information.

^b 5 cases not included because of no information.

members identified themselves as laboring class. Almost half the families in this last group have patients in the lowest of the four performance categories.

TABLE 2. PERFORMANCE LEVELS AND CLASS SELF-IDENTIFICATION

Performance Level	Class Self-Identification	
	Labor	Middle-Upper
1 (high)	20.0%	39.7%
2	29.1	33.6
3	10.9	12.1
4	40.0	14.6
N	55	116*
r_{pb}	.28	

* 11 cases not included because of no information.

The thesis that performance levels are associated with differences in the commitment of families to the dominant values of the society is supported by the relationship between performance levels and religion. Families belonging to religious groups who presumably subscribe most fully to the dominant value system—Protestants and Jews—tend to have a disproportionate number of high level patients. Ethnic differences are also re-

lated to performance levels. Among patients with foreign-born fathers, 10 of 17 whose fathers were born in Eastern Europe are found in the highest performance category, in comparison with five of 15 patients whose father were born in Ireland, and eight out of 21 patients whose fathers were born in Italy. The same relationship holds when the nationality of the mother is employed. Among patients with native-born parents, however, there are less marked differences in performance levels with respect to national origin. Since the study group was restricted to native-born patients, the number with foreign-born parents is small and these findings are somewhat tentative. The clustering of low level patients among Italian and Irish families is consistent with characterizations of their value systems, particularly the relative lack of emphasis on achievement directed activities and on the future.⁹ But the seemingly low tolerance of the Eastern European families, who are mostly Polish, is noted only as an interesting question for future research,

⁹ Cf. Florence Kluckhohn, "Family Diagnosis: Variations in the Basic Values of Family Systems," *Social Case Work*, 39 (February, 1958), pp. 63-72.

TABLE 3. PERFORMANCE LEVELS AND OBJECTIVE-SUBJECTIVE STATUS EVALUATION

I.S.C.	Class Self-Identification	Performance Level				
		1 (high)	2	3	4	N*
Under 50	Middle and Upper	45.9%	35.1	10.8	8.2	37
50 and over	Middle and Upper	36.7	32.9	12.7	17.7	79
50 and over	Labor	17.6	29.4	11.8	41.2	51
$r = .28$						

* 11 cases not included because of no information about Class Self-Identification; also 4 cases with under 50 I.S.C.—Labor Class Self-Identification are not included.

TABLE 4. PERFORMANCE LEVELS AND RELIGION

	Performance Level				N
	1 (high)	2	3	4	
Protestant-					
Jewish	45.5%	36.4%	7.2%	10.9%	55
Catholic	27.3	28.3	15.8	28.3	127

$r_{pb} = .24$

since we are not aware of empirical data directly relevant to this point.

As noted in our introductory comments, many of the themes of the dominant culture have so pervaded the society that awareness of the dominant values is found in all strata. Additional evidence of this situation is our somewhat abortive attempt to measure levels of socio-economic aspirations with a series of direct questions. In the case of several items, almost all the informants gave the most socially approved response. For example, 156 of 182 informants would prefer that a son who graduates from high school go to college rather than do something else. In the case of a few items, however, there is some indication that levels of aspiration are associated with performance levels, although the correlations are quite weak. If only families in which members identified themselves as "laboring class" are considered, the results become more suggestive; for example, among family members of patients in the highest of the four performance categories, six of 11 would take a temporary salary cut in order to get ahead in the community compared with only five of 22 relatives of patients in the lowest performance level category; similarly, seven of 11 relatives of patients with the highest performance level, compared with five of 22 with the lowest, would become more friendly with their supervisors at work so as to get ahead in the community. Admittedly, our attempt to assess levels of aspiration was less than successful, but slight trends indicate that performance levels of former patients are directly associated with the socio-economic aspirations of their families.

Thus, the data support our hypothesis of a direct relationship between performance levels of patients and the class status of their families. In addition, performance levels are directly associated with religious affiliations and with social class self-identifications of

patients' families. These relationships, as well as the more tentative associations of ethnic background and level of aspiration with performance levels, suggest that high tolerance of deviance is linked with minimal commitment to the dominant values on the part of families of former mental patients.

COMPARISON WITH THE YALE RESEARCH

Although the relationship reported here between social class and performance levels is not likely to be limited to our particular study group, support for the proposition that differential tolerance of deviance in middle and lower class families underlies this correlation is necessarily restricted by our research emphasis on families of patients who succeeded in remaining in the community. This proposition (of tolerance of deviance) could receive strong additional support by examining the social class of patients returned to the hospital. On the basis of our inference of differential tolerance and the findings presented above, we would expect a higher rate of rehospitalization among middle class than lower class patients.

The study by Hollingshead and Redlich provides data by social class on the proportion of patients who re-enter psychiatric treatment.¹⁰ In discussing their patient population, these authors report that while there is no relationship between re-entry into treatment and social class for their total patient group, there is a significant association between social class and re-entry into treatment among those persons diagnosed as schizophrenic. Schizophrenic patients in the highest social class, they indicate, are more than twice as likely to return to treatment within a year than patients in the lowest social class.¹¹ Since 82 per cent of our study

¹⁰ Hollingshead and Redlich, *op. cit.* Their data are not strictly comparable with ours, in that they employ a different class index. In the discussion that follows we employ the terms and definitions used in their volume. Thus, we refer to "re-entry into treatment" instead of rehospitalization, although, of course, treatment for most psychotic patients involves hospitalization. Professor Hollingshead read the original manuscript, and we wish to express appreciation for his comments. But we should note that they were only partially incorporated in this paper since our interpretations continue to differ in some respects from his own.

¹¹ *Ibid.*, p. 295, Table 38, cases categorized "Short Remission."

group was diagnosed as schizophrenic, these findings support the proposition of tolerance of deviance, although they suggest that the generality of this proposition may not extend to patients in other diagnostic categories, especially the organic psychoses.

In discussing the re-entry rates of the social classes in comparison with the general population (with age and sex adjusted), however, Hollingshead and Redlich report, even among schizophrenics, an *inverse* relationship between re-entry into treatment and social class.¹² On the basis of our research and their analysis of re-entry rates, we would expect exactly the opposite result, that is, a higher proportion of middle class than of lower class patients re-entering treatment.

Since their finding vitiates the proposition that tolerance of deviance is of central importance, it becomes necessary to examine in some detail the empirical facts upon which Hollingshead and Redlich base their interpretation, and, in particular, one point they note but do not discuss.¹³ They did not control for the disproportionate opportunities to re-enter treatment of members of different classes. Unless an inverse relationship between social class and re-entry into treatment can be demonstrated, taking into account the number of persons in each social class who are eligible to re-enter treatment by virtue of once having been a former patient, their interpretation of the data is misleading.

We agree with Hollingshead and Redlich that it is not possible to estimate, with precision, the number of former patients in each social class. But we do not believe that this issue should therefore be ignored. Indeed, the rates of incidence and prevalence they present indicate, relative to the probability of re-entering treatment, a *direct* relationship between class and re-entry rates—as we would expect on the basis of our own study of former mental patients.

An indication of the chances of a member of a particular class to re-enter treatment, relative to the pool of former patients of that class, can be calculated by dividing the re-entry rates by the incidence rates.¹⁴ When

this is done, the class differential disappears—except that the re-entry rate of classes 1 and 2 is between 25 and 50 per cent *higher* than those of the other social classes. This new statistic probably still overestimates the relative re-entry rate of the lower classes. For a less conservative estimate, prevalence rates can be used; this procedure underestimates somewhat the proportionate chances of a lower class person to re-enter treatment, since we do not control differences in age at onset of illness and a number of other variables. Using prevalence rates in the same manner as incidence rates were employed, however, results in a marked *direct* relationship between re-entry and social class, suggesting that the re-entry rate of the two highest social classes may be as much as four times that of the lowest social class.¹⁵

The higher tolerance of deviant behavior by the lower class that is indicated by our research as well as the Yale study, if also present prior to hospitalization, serves to amplify certain of the other findings of the latter investigation. For example, it is reported that among patients of private practitioners, as well as among those in clinics and hospitals, a high correlation exists between type of treatment and social class. Characteristically psychotherapy is prescribed for upper class patients and shock treatment for those of the lower class. Undoubtedly, as the Yale group and others have pointed out, this is a function of class differences in the practitioner-patient relationship, types of treatment institutions to which patients of various classes are referred, and ability to pay. If lower classes are more tolerant of deviant behavior, however, patients from these families are probably “sicker,” in the sense of manifesting more bizarre behavior, when first referred for treatment. Thus, they may receive shock treatment rather than psychotherapy, partly because of being more acutely ill (in the sense noted above).

Similarly, the findings of Hollingshead and Redlich that lower class psychotic patients are under treatment for more years than

¹² *Ibid.*, pp. 235–236.

¹³ *Ibid.*, p. 219, fn. 26.

¹⁴ *Ibid.*, p. 235, Table 16. The results are similar when all psychotics or only schizophrenics are analyzed.

¹⁵ We are grateful for the advice of Sol Levine and Robert B. Reed concerning this procedure. Of course, they are in no way responsible for the interpretation presented in this paper.

higher class patients may, to some extent at least, be a function of the development of graver illness before their more tolerant families sought treatment for them. Indeed, our inference of differential tolerance of deviance strengthens the Hollingshead and Redlich basic thesis of a close relationship between mental illness and social class. If lower class families are more tolerant of deviance, the strong association they report between incidence of psychosis (when only *treated* patients are considered) and social class actually underestimates the proportion of the lower classes who are psychotic, since they must manifest more acute symptoms of illness before being referred for treatment.¹⁶

SUMMARY

The foregoing discussion was initiated by depiction of the general thesis that the differential commitment of relatives to the dominant values of the society has substantial consequences for the posthospital fate of the mental patient. Evidence has been presented to support the hypothesis of a direct relationship between patients' posthospital performance levels and the social class status of their families. This hypothesis, one of a series specifying relationships between pa-

tients' posthospital performance levels and certain variables pertaining to differences in family structure and in the personality characteristics of family members, is based on the proposition that the latter's tolerance of deviant behavior is a key factor affecting the course of a patient's posthospital experience.¹⁷

Our findings provide support for the hypothesized relationship between class status and performance levels. In addition to measures of objective class status, the relationships between performance levels and three other variables (class self-identification, religion, and ethnic background) generally believed to reflect degree of commitment to the dominant values are all in the predicted direction. The correlation between performance levels and ethnic background, however, must be regarded as tentative.

These findings regarding social class, and those of the Yale study considered in this paper, provide further support for the proposition that the differential tolerance of family members is critical to the posthospital fate of the mental patient.

¹⁶ Our research does not control for the influence of prehospital performance level or differential effectiveness of hospitalization. With respect to certain of our other findings, these variables received limited consideration; analysis of the prehospital and hospital conditions do not vitiate our proposition of tolerance as a key factor in the posthospital experience of former mental patients. Cf. "Mental Patients in the Community: Family Settings and Performance Levels," *op. cit.*, pp. 152-153.

¹⁷ We have not attempted to relate the findings regarding social class to our previously reported correlates of performance levels. Many of these—reflecting structural differences among families and personality characteristics of family members—are, of course, associated with social class. An examination of the interrelationships between social class and these other correlates indicates that, in a statistical sense, their associations with performance levels are not artifacts of each other. On a conceptual level, however, we have not yet thoroughly exploited the data, and the various findings need to be more fully integrated. We acknowledge therefore the incompleteness of our research task.

SOCIAL CLASS AND THE EXERCISE OF PARENTAL AUTHORITY *

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The conditions under which middle- and working-class parents punish their pre-adolescent children physically, or refrain from doing so, appear to be quite different. Working-class parents are more likely to respond in terms of the immediate consequences of the child's actions, middle-class parents in terms of their interpretation of the child's intent in acting as he does. This reflects differences in parents' values: Working-class parents value for their children qualities that assure respectability; desirable behavior consists essentially of not violating proscriptions. Middle-class parents value the child's development of internalized standards of conduct; desirable behavior consists essentially of acting according to the dictates of one's own principles. The first necessarily focuses on the act itself, the second on the actor's intent.

MUCH past research on the relationship between social class and the exercise of parental authority has been concerned with the question of whether or not working-class parents typically employ different techniques from those used by middle-class parents in dealing with their children's misbehavior.¹ Bronfenbrenner has summarized the results of twenty-five years of investigation in this area as indicating that: "In matters of discipline, working-class parents are consistently more likely to employ physical punishment, while middle-class families rely more on reasoning, isolation,

appeals to guilt, and other methods involving the threat of loss of love."²

The studies to which Bronfenbrenner refers have relied primarily on parents' generalized statements about their usual or their preferred methods of dealing with discipline problems—irrespective of what the particular problem might be. But obviously not all discipline problems evoke the same kinds of parental response. In some sense, after all, the punishment fits the crime. *Under what conditions* do parents of a given social class punish their children physically, reason with them, isolate them—or ignore their actions altogether?

The present study attempts to specify the practices of middle- and working-class parents in various circumstances, and from this information to develop a more general interpretation of the relationship of social class to the exercise of parental authority.

SAMPLE AND METHOD OF DATA COLLECTION

Washington, D. C.—the locus of this study—has a large proportion of people employed by government, relatively little heavy industry, few recent immigrants, a white working class drawn heavily from rural

* A revised version of a paper presented at the annual meeting of the American Sociological Society, August, 1958. This is the second report of a more general inquiry into the relationship of class and family directed by the author and John A. Clausen, with the collaboration and aid of Eleanor Carroll, Bernard Finifter, Mary Freeman, Paul Hanlon, Sylvia Marshall, Alexander Shakow, and Eleanor Wolff. Expert advice on sample design and statistical problems was generously provided by Samuel W. Greenhouse. The interviews were conducted during the period March, 1956 to March, 1957.

¹ Sometimes the question has been put as to whether or not working-class parents employ more "severe" techniques. This of course involves an *a priori* judgment as to which techniques are most "severe."

Another major question, arising out of studies of the training of infants and very young children (e.g., weaning, toilet training) has been: which social class is the more "permissive," which the more "rigid," in its training procedures? These terms lead to difficulty because of the ambiguity as to what behaviors may be regarded as constituting "permissiveness" and "rigidity."

² Urie Bronfenbrenner, "Socialization and Social Class Through Time and Space," in Eleanor E. Maccoby, Theodore M. Newcomb, and Eugene L. Hartley, editors, *Readings in Social Psychology*, New York: Holt, 1958, p. 424. This article provides a fine analytic summary of past research on class and family, as well as a bibliography of the major studies in this field.

areas, and a large proportion of Negroes, particularly at lower economic levels. Generalizations based on this or any other sample of one city during one limited period of time are, of course, tentative.

Our intent in selecting the families to be studied was to secure approximately two hundred representative white working-class families and another two hundred representative white middle-class families, each family having a child within a narrowly delimited age range. We decided on fifth-grade children in order to be able to direct the interviews to relationships involving a child old enough to have a developed capacity for verbal communication.

The sampling procedure involved two steps: the first, selection of Census tracts. Tracts with 20 per cent or more Negro population were excluded, as were those in the highest quartile with respect to median income. From among the remaining tracts we then selected four with a predominantly working-class population, four predominantly middle-class, and three having large proportions of each. The final selection of tracts was based on their occupational distribution and their median income, education, rent (of rented homes), and property value (of owner-occupied homes). The second step in the sampling procedure involved selection of families. From records made available by the public and parochial school systems, we compiled lists of all fifth-grade children whose families lived in the selected tracts. Two hundred families were then randomly selected from among those in which the father had a "white-collar" occupation and another two hundred from among those in which the father had a manual occupation.

In all four hundred families, the mothers were to be interviewed. In every fourth family we also scheduled interviews with the father and the fifth-grade child. (When a broken family fell into this sub-sample, a substitute was chosen from our overall sample, but the broken family was retained in the overall sample.)

When interviews with both parents were scheduled, two members of the staff visited the home together—a male to interview the father, a female to interview the mother. The interviews were conducted independently, in separate rooms, but with essentially identical

schedules. The first person to complete his interview with the parent interviewed the child.

We secured the cooperation of 86 per cent of the families where the mother alone was to be interviewed, and 82 per cent of the families where mother, father, and child were to be interviewed. Rates of non-response do not vary by social class, type of neighborhood, or type of school. This of course does not rule out other possible selective biases introduced by the non-respondents.

Index of Social Class. Each family's social class position was determined by the Hollingshead Index of Social Position, assigning the father's occupational status a relative weight of 7 and his educational status a weight of 4. Here we consider Hollingshead's Classes I, II, and III to be "middle-class," and Classes IV and V as "working-class." The middle-class sample is composed of two fairly distinct groups: Classes I and II are almost entirely professionals, proprietors, or managers, with at least some college training. Class III is made up of small shopkeepers, clerks, and salespersons, but includes a small number of foremen and skilled workers of unusually high educational status. The working-class sample is composed entirely of manual workers, but preponderantly those of higher skill levels. These families are of the "stable working class" rather than "lower class" in the sense that the men have steady jobs and in that their education, income, and skill levels are above those of the lowest socio-economic strata.

THE CONTEXT OF AUTHORITY

The context for our study of parents' reactions to specific disciplinary situations is provided by a cursory examination of three general aspects of authority: the relative role of mother and father in making family decisions, the relative role of mother and father in setting limits upon the children's freedom of movement or activity, and the frequency with which mother and father resort to physical punishment to enforce obedience. From none of these perspectives do we find any appreciable difference between middle- and working-class families. Middle-class parents' and children's evaluations of the extent to which each parent participates in the making of day-to-day de-

TABLE 1. MOTHERS' EVALUATIONS OF THE DECISION-MAKING PROCESS IN MIDDLE- AND WORKING-CLASS FAMILIES

Type of decision	Proportion of Mothers Who Report that:							
	Decisions are Made Primarily by:				Parent Participates in Decisions:			
	Mother		Father		Mother		Father	
	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class
Day-to-day decisions	.90	.82	.04	.05	.97	.95	.12	.20
Major family decisions	.05	.14	.22	.19	.81	.82	.95	.88
Decisions that affect the fifth-grade child most directly	.38	.43	.06	.08	.98	.95	.71	.68

Number of cases: 163 middle-class and 146 working-class. (This table is limited to mothers living with their husbands; the total sample is composed of 174 middle-class and 165 working-class families.)

cisions, major family decisions, and the decisions that affect the fifth-grade child most directly, are quite similar to those of working-class parents and children (see Table 1).³ Nor is there any appreciable difference between the social classes in mothers', fathers', or children's evaluations of which

parent is stricter, more likely to restrict the children's freedom, "lay down the law" when the child misbehaves, or dominate the child (see Table 2). Finally, middle-class parents report that they make use of physical punishment about as frequently as do working-class parents (see Table 3).

Nevertheless, there are distinct differences in the conditions under which middle- and working-class parents resort to physical punishment. We shall see that parents of both social classes reserve physical punishment for fairly extreme circumstances. But even in these extreme circumstances, some actions that are intolerable to working-class parents are not punished by middle-class parents, and other actions intolerable to

³ Tables 1 and 2 present only the mothers' evaluations. The evaluations provided by fathers and children yield entirely consistent results. (Although there are no appreciable differences between middle- and working-class families with respect to which parent predominates in decision-making and limit-setting, it may be that working-class respondents are less likely to perceive the two parents as sharing equally in these functions. This question will be examined in another report.)

TABLE 2. MIDDLE- AND WORKING-CLASS MOTHERS' EVALUATIONS OF THEIR OWN AND THEIR HUSBANDS' ROLES IN SETTING LIMITS UPON THEIR CHILDREN'S FREEDOM OF MOVEMENT OR ACTIVITY

Proportion of Mothers Who Report That:	Parents' Behavior Directed to:			
	Sons		Daughters	
	Middle Class	Working Class	Middle Class	Working Class
Mother is stricter than father	.32	.37	.32	.25
Father is stricter than mother	.44	.36	.40	.38
Mother is more likely to restrict child's freedom	.45	.39	.38	.36
Father is more likely to restrict child's freedom	.29	.40	.31	.29
Mother is more likely to "lay down the law" when child misbehaves	.34	.34	.43	.37
Father is more likely to "lay down the law" when child misbehaves	.37	.33	.27	.40
Mother is more likely to dominate the child	.34	.32	.41	.45
Father is more likely to dominate the child	.22	.16	.17	.21
Number of cases *	82	73	81	73

* Limited to intact families.

TABLE 3. THE REPORTED USE OF PHYSICAL PUNISHMENT BY MIDDLE- AND WORKING-CLASS PARENTS

	Proportion of parents Who Report Using Physical Punishment:							
	Mothers				Fathers			
	Response to Sons		Response to Daughters		Response to Sons		Response to Daughters	
	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class
Parent reports that he (or she) punishes child physically:								
Occasionally or frequently	.14	.16	.09	.16	.04	.04	.10	.00
Infrequently	.46	.44	.39	.42	.40	.40	.33	.36
Parent reports that he (or she) last punished child physically:								
Within past week	.16	.13	.13	.14	.00	.12	.14	.09
Within past month	.30	.28	.27	.32	.16	.28	.19	.27
Within past six months	.59	.55	.45	.56	.40	.44	.43	.36
Number of cases *	79	82	75	77	25	25	21	11

* These questions were not asked of the first 26 mothers interviewed.

middle-class parents are not punished by working-class parents.

In attempting to specify the conditions under which middle- and working-class parents use physical punishment, we rely here on parent's reported reactions to eight types of situation: the child's wild play, fighting with his brothers or sisters, fighting with other children, really losing his temper, refusing to do what his parent tells him to do, "swiping" something from home or from other children, smoking cigarettes, and using language his parent doesn't want him to use.

Parents were questioned in some detail about each of these situations. We asked, for example, whether or not the fifth-grade child ever loses his temper; precisely what he does when he loses his temper; what the parent "generally does when he acts this way;" whether he "ever finds it necessary to do anything else" and, if so, "under what circumstances" and what else he does.⁴

⁴ Middle- and working-class mothers were equally likely to report that their children have lost their tempers, fought with others, refused to do what they were told, swiped something, smoked, or used disapproved language. Middle-class mothers, however, were more likely to tell us that their sons had played wildly (71 per cent vs. 52 per cent) and fought with their brothers or sisters (97 per cent vs. 87 per cent of those having brothers or sisters close

Parents' reports on their reactions to their children's behavior were classified according to the following scheme:

1. Ignore: not doing anything about it.

2. Scold, admonish to be good, demand that he stop, inquire into causes of behavior, scream at him, threaten to punish him. (It has proved impossible to differentiate these several verbal reactions reliably from interview material.⁵ We could not determine, for

to them in age). This does not mean that a disinterested observer would agree that working-class boys are less likely than are middle-class boys to play wildly or fight with their brothers or sisters, or that working-class mothers have withheld information from the interviewers. Instead, it reflects the fact that mothers who said that these situations do *not* occur conceive of "wild play" in terms that we would classify as *aggressively* wild play (not boisterousness) and fighting in terms that we would classify as *physical combat* (not an argument).

In general, middle- and working-class mothers who say that any given situation does occur were likely to describe their children's specific actions in quite similar ways. Two notable exceptions are that working-class mothers were more apt to describe their sons' fights with brothers or sisters and their daughters' fights with other children as physical encounters, while middle-class mothers were more likely to describe them as arguments.

⁵ The standard of coding reliability used here and throughout the study was 90 per cent inter-coder agreement on independent trials for each classification used.

TABLE 4. MOTHERS' REPORTED RESPONSES TO THEIR CHILDREN'S ACTIONS—UNDER USUAL CIRCUMSTANCES
(Limited to mothers who report that their children behave in the particular way)

	Proportion of Mothers Who Report that They Respond by:												Number of Cases			
	Ignoring				Scolding or Admonishing		Separating the Child from Others		Punishing Physically		Isolating or Restricting Activities					
	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class		
When child:																
1. Plays wildly																
Sons	.31	.34	.42	.45	.17	.07	.00	.02	.10	.12	64	44				
Daughters	.36	.56	.49	.28	.10	.08	.00	.03	.05	.05	42	39				
2. Fights with his brothers or sisters																
Sons	.13	.05	.54	.40	.18	.31	.02	.02	.13	.22	65	55				
Daughters	.18	.10	.53	.60	.19	.08	.02	.06	.08	.16	68	50				
3. Fights with children other than brothers or sisters																
Sons	.48	.44	.37	.36	.15	.08	.00	.00	.00	.12	46	50				
Daughters	.35	.29	.48	.46	.17	.19	.00	.00	.00	.06	42	41				
4. Loses his temper																
Sons	.32	.30	.49	.41	.03	.01	.05	.09	.11	.19	81	74				
Daughters	.41	.32	.40	.43	.00	.00	.06	.11	.13	.14	67	57				
5. Refuses to do what his mother tells him to do																
Sons	.14	.19	.74	.77	.00	.02	.00	.00	.12	.02	56	43				
Daughters	.30	.09	.59	.77	.00	.00	.00	.06	.11	.08	37	35				
6. Uses language that his mother doesn't want him to use																
Sons	.11	.03	.78	.86	.00	.00	.00	.05	.11	.06	48	36				
Daughters	.04	.00	.89	.78	.00	.00	.00	.04	.07	.18	29	27				
7. Smokes cigarettes																
Sons	.24	.27	.66	.73	.00	.00	.00	.00	.10	.00	21	15				
Daughters	.64	.30	.36	.50	.00	.00	.00	.10	.00	.10	14	10				
8. Swipes something from home or from other children																
Sons	.12	.05	.76	.90	.04	.00	.00	.00	.08	.05	26	20				
Daughters	.00	.08	.67	.67	.00	.00	.22	.08	.11	.17	9	12				

* Social class differences statistically significant, .05 level or better, using chi-square test.

example, whether or not a parent's reported attempt to discover the causes of a fight was in fact a scolding. Therefore we have reluctantly decided to treat these several responses as a single category.)

3. Separate from other children or divert attention: removing the child from the situation or providing alternative activities.

4. Punish or coerce: (a) Punish physically—everything from a slap to a spanking. (b) Isolate—confining child *alone* for a period of time, for example, sending him to bed during the day. (c) Restrict usual activities—limiting his freedom of movement or activity short of isolation, for example, not letting him play outside.

THE EXERCISE OF MATERNAL AUTHORITY

Most mothers—in both social classes—report that their *usual* response in the eight situations about which we inquired is to ignore the child's actions altogether, or at most to admonish him (see Table 4). Few mothers isolate or restrict their children at this stage of things, and virtually none punishes them physically. One can not conclude that mothers of either social class are especially quick to resort to physical punishment or to other forms of coercion.

But when their children persist in wild play, fighting with their brothers or sisters, or displays of temper, both middle- and working-class mothers are apt to turn to one or another form of punishment (see Table 5). Working-class mothers are more likely than are middle-class mothers to do so in the case of their sons' prolonged loss of temper; middle-class mothers, on the other hand, are more likely to punish their sons for refusing to do as they are told.⁶

Working-class mothers are more likely than are middle-class mothers to resort to *physical* punishment when their sons persist in wild play or fighting with brothers or sisters, or when their daughters fight with other children. There may be in addition a general, albeit slight, greater tendency for working-class mothers to resort to physical punishment no matter what the situation. (In thirteen of sixteen comparisons, a some-

what larger proportion of working-class than of middle-class mothers report using physical punishment, although only in the three comparisons noted above is the difference large enough for us to be confident that it is not simply a chance occurrence.) But we previously noted that middle-class mothers say they use physical punishment about as *frequently* as do working-class mothers. Therefore, it is not likely that working-class mothers' propensity to use physical punishment is sufficiently greater than that of middle-class mothers to be of serious import. A more important difference lies in the conditions under which mothers of the two social classes punish their children physically.

The Conditions under Which Working-Class Mothers Punish Their Sons Physically. Working-class mothers are apt to resort to physical punishment when the immediate consequences of their sons' disobedient acts are most extreme, and to refrain from using punishment when its use might provoke an even greater disturbance.

We have noted two actions for which working-class mothers are more likely than are middle-class mothers to punish their sons physically: wild play and fighting with their brothers or sisters. In either situation, the more extreme their sons' actions, the more likely are working-class mothers to use physical punishment (see Table 6). Those whose descriptions of their sons' wild play indicate that it is nothing more than boisterousness or running around are no more apt to resort to physical punishment than are middle-class mothers in the same circumstances. But those whose descriptions of wild play include elements that we see as aggression or destruction are far more likely than are middle-class mothers to employ physical punishment. Similarly, working-class mothers are not appreciably more likely than are middle-class mothers to punish their sons physically for fights with brothers or sisters when the "fights" are no more than arguments, but they are more likely to resort to physical punishment when the fights involve physical combat.

This suggests that the more extreme forms of wild play and fighting are particularly intolerable to working-class mothers, and less so to middle-class mothers. This impression is sustained by the fact that those working-

⁶ The criterion of statistical significance used throughout this paper is the five per cent level, based on the chi-square test.

TABLE 5. MOTHERS' REPORTED USE OF PHYSICAL PUNISHMENT, ISOLATION, AND RESTRICTION—IN EXTREME CIRCUMSTANCES
(Limited to mothers who report that their children behave in the particular way)

When child:	Proportion of Mothers Who Report that They Use:										Number of Cases	
	Physical Punishment			Isolation		Restriction		Punishment of Any Type				
	Middle Class	Working Class		Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	
1. Plays wildly												
Sons	.09	.23		.05	.02		.20	.18		.34	.43	
Daughters	.02	.08		.12	.00		.17	.18		.31	.26	
2. Fights with his brothers or sisters												
Sons	.17	.43		.34	.25		.06	.04		.57	.72	
Daughters	.21	.26		.24	.26		.09	.14		.54	.66	
3. Fights with children other than brothers or sisters												
Sons	.00	.02		.00	.06		.15	.12		.15	.20	
Daughters	.00	.15		.02	.05		.14	.12		.16	.32	
4. Loses his temper												
Sons	.20	.30		.14	.11		.06	.19		.40	.60	
Daughters	.24	.26		.12	.12		.03	.09		.39	.47	
5. Refuses to do what his mother tells him to do												
Sons	.12	.07		.04	.07		.18	.00		.34	.14	
Daughters	.08	.23		.05	.06		.14	.06		.27	.35	
6. Uses language that his mother doesn't want him to use												
Sons	.04	.11		.02	.00		.10	.00		.16	.11	
Daughters	.00	.11		.03	.00		.07	.11		.10	.22	
7. Smokes cigarettes												
Sons	.00	.00		.00	.00		.05	.00		.05	.00	
Daughters	.00	.10		.00	.00		.00	.10		.00	.20	
8. Swipes something from home or from other children												
Sons	.04	.00		.00	.00		.08	.05		.12	.05	
Daughters	.12	.17		.11	.00		.00	.25		.23	.42	

* Social class differences statistically significant, .05 level or better, using chi-square test.

TABLE 6. MOTHERS' REPORTED USE OF PHYSICAL PUNISHMENT, ISOLATION, AND RESTRICTION, IN EXTREME CIRCUMSTANCES—
CONTROLLED ON MOTHER'S DESCRIPTION OF HER CHILD'S BEHAVIOR

Proportion of Mothers Who Report that They Use:										
When son:	Physical Punishment		Isolation		Restriction		Punishment of Any Type		Number of Cases	
	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class
1. Plays wildly										
a. Described as boisterous- ness or running around	.13	.11	.03	.04	.16	.18	.32	.33	38	28
b. Described as willful aggression or destruction	.04	*	.43	.00	.26	.19	.38	.62	26	16
2. Fights with his brothers or sisters										
a. Described as argument	.10	.27	.30	.13	.10	.13	.50	.53	30	15
b. Described as physical encounter	.23	*	.50	.30	.03	.00	.63	.80	35	40
3. Fights with children other than his brothers or sisters										
a. Described as physical encounter	.00	.02	.00	.07	.16	.12	.16	.21	31	42
4. Loses his temper										
a. Described as pouting, yelling, or sulking	.15	.26	.12	.06	.06	*	.33	*	66	54
b. Described as a violent or aggressive outburst	.40	.40	.20	.25	.07	.10	.67	.75	15	20

* Social class differences statistically significant, .05 level or better, using chi-square test.

class mothers who consider themselves unusually strict and "ready to lay down the law" are especially likely to punish their sons physically for physical combat with brothers or sisters. And those working-class mothers who describe themselves as easily angered by their sons' actions or unwilling to "give in" to them are especially likely to use physical punishment for aggressively wild play. They cannot or will not tolerate these forms of aggressive behavior.

But working-class mothers do not find all aggressive behavior intolerable. They are far less likely to punish their sons physically for fights with friends or neighbors than for equally serious fights with brothers or sisters. It would appear that it is not for aggressive behavior as such, but for the disturbances arising out of aggressive behavior, that working-class boys are punished. Their mothers seem unwilling or unable to tolerate the immediate consequences of their sons' aggressive acts.

The responsiveness of working-class mothers to immediate consequences is demonstrated anew by a consideration of the conditions under which they do *not* punish their sons. They shun punishment if it might provoke a disturbance more serious than that already underway.

There is ample evidence presented above that working-class mothers are prone to punish their sons physically for acts of disobedience. Furthermore, they are more likely than are middle-class mothers to report that

on the last occasion they used physical punishment it was invoked in response to disobedience. But there is a world of difference between punishing a boy for violating a negative injunction and punishing him for not doing something he is positively enjoined to do. Working-class mothers appear to be far more likely to punish their sons for the former type of disobedience than for the latter. In fact, they are less likely than are middle-class mothers to punish their sons for refusing to do things they have been told to do (see Table 5).

The immediate consequences of acquiescing to a son's refusal may be trivial—often nothing more than doing a minor household chore when he will not comply. However, under some conditions—specifically, when the boy is adamant about his refusal—the consequences of forcing him to do as he is told may be serious. Working-class mothers are highly unlikely to punish their sons—physically or in any other way—in such circumstances (see Table 7). No working-class mother who described her son's refusals as prolonged delays under conditions where he had unquestionably heard the order, or as an act of outright defiance, told us that she punished him physically, restricted his activities, or isolated him. They were more likely than were middle-class mothers to tell us that they took no action at all under these circumstances. Nor does this indicate indifference. The interview reports indicate that these mothers do make an at-

TABLE 7. MOTHERS' REPORTED RESPONSES WHEN THEIR CHILDREN DEFIANTLY REFUSE TO DO WHAT THEY ARE TOLD TO DO

By:	Proportion of Mothers Who Report that They Respond:			
	To Sons		To Daughters	
	Middle Class	Working Class	Middle Class	Working Class
Punishing them physically	.13	*	.09	.25
Isolating them	.02		.04	.07
Restricting their activities	.17		.12	.07
Scolding, admonishing, insisting that child obey, threatening to punish	.59	.66	.63	.57
Not doing anything about it	.09	*.34	.12	.04
	1.00	1.00	1.00	1.00
Number of cases	46	32	33	28

* Social class differences statistically significant, .05 level or better, using chi-square test.

tempt to secure compliance, but then back down. This is especially true of those who say they are "easily upset" by their sons' actions. They seem unable to bring themselves to take strong action, but are hardly indifferent.

The Conditions under Which Middle-Class Mothers Punish Their Sons Physically. It is clear that middle-class and working-class mothers make different discriminations in their use of physical punishment. Middle-class mothers seem to punish or refrain from punishing on the basis of their interpretation of the child's intent. Most indicative of this are their responses to wild play and loss of temper (see Table 6). They are not likely to punish their sons physically for wild play, however serious it may be. Nor are they particularly likely to punish them physically for loss of temper when it is manifested only as pouting, yelling, or sulking. But a violent or aggressive outburst of temper is far more likely to elicit physical punishment. In these circumstances, middle-class mothers are as prone as their working-class counterparts to punish their sons physically.⁷

The descriptions of temper-loss classified here as "a violent or aggressive outburst" are quite similar to the descriptions of wild play classified as "willful aggression or destruction." Working-class mothers respond to the one much as they do to the other. But middle-class mothers are far more likely to punish their sons physically for what they call loss of temper than for behavior defined as wild play. They appear to find the child's loss of temper, but not his wild play, particularly intolerable.

There is one salient respect in which an outburst of temper may be quite different from wild play: the outburst may be directed

against the mother herself. Short of a frontal assault, however, this is largely a matter of the mother's interpretation. The interview reports indicate that the distinction between wild play and loss of temper was most often made in terms of the child's presumed intent, as judged by his preceding actions. If in the course of play he became very excited, this was not judged to be loss of temper—however extreme his actions. But if his actions seemed to stem from the frustration of not having his own way, they were judged to indicate loss of temper. The overt behavior in the two types of situation might be, and often was, nearly identical.

The Conditions under Which Mothers Punish Their Daughters. Middle-class mothers appear to respond to their daughters' actions much as they do to their sons' (see Tables 5 and 7). But working-class mothers are more likely to use physical punishment when their sons play wildly or fight with brothers or sisters than when their daughters do so (see Table 5). This tendency, however, reflects the fact that boys' wild play and fighting are more apt to be extreme. Daughters are only slightly less likely to be punished physically for behavior which is actually similar. They are, in fact, *more* likely to be punished physically when they swipe something or fight with children other than brothers or sisters. (Nevertheless, working-class mothers are far less likely to punish their daughters physically for fights with friends than for equally serious fights with brothers or sisters. In this crucial respect, their treatment of boys and of girls is much the same.)

The most dramatic difference in the working-class mothers' response to boys and to girls occurs when the child defiantly refuses to do as he is told: boys are permitted to have their own way, while girls are punished physically (see Table 7). Something more is expected of a girl than of a boy—she must not only refrain from doing what she's not supposed to do, but must also carry out actions her mother wants her to do.

There is no indication in these data that working-class mothers are more prone to examine their daughters' than their sons' intent, but clearly, the actions are evaluated differently. We shall return to the question of why this is so.

⁷ We noted earlier (with reference to Table 5) that working-class mothers are more likely than are middle-class mothers to punish their sons for loss of temper. It is now clear that this difference obtains almost entirely among mothers who describe the child's loss of temper as pouting or yelling, for middle-class mothers are nearly as likely as are working-class mothers to punish a violent or aggressive outburst. This, too, we take as evidence (admittedly indirect) that working-class mothers are attuned to immediate consequences. Although yelling or pouting may not be of very serious import (in the sense that the child does not necessarily lose control over his actions), it is disconcerting, and working-class mothers do not abide it.

Hypothetical Reactions. Whenever a mother told us that her child had *not* performed an action about which we inquired, we asked her what she thought she would do if the situation did occur. Middle-class mothers who said that a given situation had not occurred thought that they would probably respond in ways almost identical to those noted by mothers who said that it had taken place. This is not the case for working-class mothers. Although no working-class mother who declared that her son had swiped something, smoked, or defiantly refused to carry out an order reported punishing him physically, roughly one-fifth of those who said that their sons had not done these things expected to use this sanction if the situation were to occur.

It is possible, of course, that those working-class mothers who are most prone to use physical punishment are unwilling to admit that their sons misbehave, or have somehow forestalled their sons' misbehavior. This does not seem likely, however, for in circumstances when working-class mothers are quite likely to use physical punishment—when their sons engage in aggressively wild play—those who say that they have not faced the situation are quite unlikely to predict that they would resort to physical punishment. (Roughly one-fifth of such mothers think that they would do so—about the same proportion as those who think that they would punish their sons physically for swiping something, smoking, or being defiant.)

It seems, then, that working-class mothers are unable to envisage either the conditions under which they would be very likely to use physical punishment or the conditions under which they would be very unlikely to do so. Considering the degree to which they are responsive to immediate circumstances, this conclusion should not be surprising.

PATERNAL AUTHORITY

In considering fathers' reactions to their children's behavior, we rely on interviews both with a sub-sample of eighty-two fathers and the much larger sample of mothers. The small number of interviews with fathers limits us severely in attempting to take account of the sex of the child and the father's description of the child's behavior. Thus we

are forced to interweave the analysis of fathers' self-reports with wives' reports on their behavior. This procedure is not entirely satisfactory, for reasons presented in the next section.

Working-class fathers are appreciably less likely than are their wives to say that they punish their sons physically for wild play (see Tables 5 and 8). In fact, they are no more likely than are middle-class fathers to report using physical punishment in this situation. This reflects the fact that few working-class fathers describe their sons' wild play as anything more than boisterousness—for which they are most unlikely to punish boys physically. However, those few working-class mothers who indicate that their husbands are exposed to *aggressively* wild play are far more likely than are middle-class mothers to report that in these extreme circumstances their husbands resort to physical punishment. It would appear that working-class fathers respond in two ways: if the child's behavior does not compel their attention, they are apt to ignore it; but if it is sufficiently disruptive, they are very likely to use physical punishment. They may be even more responsive to immediate consequences than are their wives.

Middle-class fathers apparently are a good deal more likely than are their wives to punish sons physically for fighting with their brothers or sisters (see Tables 5 and 8). This seems to be true however serious the fights. Thus, middle-class fathers are as likely as are working-class fathers to resort to physical punishment when their sons fight. Nevertheless, working-class fathers are somewhat more likely to report using physical punishment, and considerably more likely to report using isolation or restriction, when their sons' fights are *serious*. The wives' reports lead to the same conclusion.

In other respects as well, our information indicates that the major conclusions about the conditions under which mothers of the two social classes punish their children physically, noted above, also apply to fathers. In particular, middle-class mothers say that their husbands are far more likely to punish their sons physically for severe loss of temper than for aggressively wild play. (The small number of fathers' reports on reactions to

TABLE 8. FATHERS' REPORTED RESPONSES TO THEIR SONS' ACTIONS—UNDER EXTREME CIRCUMSTANCES
(Limited to fathers who report that their sons behave in the particular way)

When son:	Proportion of Fathers Who Report that They Use:						Number of Cases Middle Working Class			
	Physical Punishment		Isolation		Restriction				Punishment of Any Type	
	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class	Middle Class	Working Class		
1. Plays wildly	.07	.06	.07	.06	.07	.17	.21	.29	15	18
2. Fights with his brothers or sisters	.43	.50	.10	.25	.05	.06	.58	.81	21	16
3. Fights with children other than brothers or sisters	.00	.00	.00	.00	.00	.00	.00	.00	15	11
4. Loses his temper	.22	.16	.13	.11	.00	.16	.35	.43	23	19
5. Refuses to do what his father tells him to do	.19	.20	.06	.00	.13	.10	.38	.30	16	10

* Social class differences statistically significant, .05 level or better, using chi-square test.

severe loss of temper prevents our determining whether or not fathers agree.) Working-class mothers, furthermore, tell us that their husbands are not likely to punish their sons' defiant refusals to do as they are told, but are likely to punish their daughters physically for similar behavior. (The few fathers' reports relevant to this issue are consistent with this finding.)

STRENGTHS AND LIMITATIONS OF THE DATA

Although we have reports from all three relevant persons in a representative subsample of these families, there remains the question as to whether or not disinterested observers would present similar descriptions. Moreover, interview responses are in their nature limited: we cannot differentiate reliably among various types of verbal behavior; we cannot tell whether or not a parent expresses his displeasure by grouching, assuming an air of martyrdom, or simply "acting differently;" and we do not know to what degree any of a parent's actions may be interpreted by his child as a withdrawal of love.

Each respondent has summarized a number of his own and another's actions. We do not know how frequently situations of a given type have occurred,⁸ or how consistent a parent's reactions have been. Nor can we, for example, assume from a parent's description of his child's fights as arguments, that the child never fights physically.

On the other hand, the parents' ability to differentiate between what they generally do and what they do in more pressing circumstances gives reason for confidence in the findings. So does the fact that the relationships we have found between social class and mothers' reactions are not appreciably modified by controlling other relevant variables (including mother's age, size of the family, ordinal position of the child in the family, length of time the family has lived in the neighborhood, the socio-economic status of the neighborhood, whether or not the mother has a job, and if so what type of

⁸ It is primarily for this reason that one cannot draw from our data on parents' reactions to specific types of situations the inference that working-class parents use physical punishment more often than do middle-class parents.

job, whether or not she has been socially mobile or feels socially mobile, her social class identification, her level of education, her religious background, whether her background is rural or urban, whether or not she reads popular literature on child-rearing, and whether or not her husband works for the government.)

Furthermore, the information provided by the sub-sample of fathers and children enables us to check some of the inferences we have drawn from mothers' reports.⁹ We asked each father, for example, "What does your wife generally do when [child] fights with his brothers or sisters?" Although the question parallels "What do *you* generally do . . . ?," it was put immediately after we asked him what he does in circumstances other than those that usually prevail. Most parents seem to have answered with the latter circumstances in mind. This precludes a comparison of what fathers say they do with what they say their wives do in exactly the same circumstances. But it does give us some basis for judging whether or not our inferences about social class differences in the conditions under which mothers use physical punishment are supported by what husbands tell us.

The only respect in which the fathers' reports of their wives' reactions raise a question about the inferences we have drawn is that two of the nine working-class fathers who say that their sons defiantly refuse to do as they are told also report that their wives punish the boys. Perhaps, then, we have overstated the case in concluding that working-class mothers are singularly unlikely to punish their sons for defiance.

Concerning the fathers' reactions, it appears that interviews with fathers and with mothers yield much the same conclusions. This holds even for the conclusion that working-class fathers are unlikely to punish their sons for a defiant refusal.

The children, too, were asked about their parents' reactions in three of the situations: fights with brothers and sisters, fights with other children, and loss of temper. The information provided by middle-class children, although based on few interviews, is entirely

consistent with their parents' reports. But working-class boys are rather unlikely to tell us that their mothers punish them *physically* for fighting with brothers or sisters or for loss of temper. They acknowledge that their fathers do so, but they report that their mothers isolate or restrict them for such actions.

INTERPRETATION

Neither middle- nor working-class parents resort to punishment as a first recourse when their children misbehave. It seems, instead, that parents of both social classes initially post limits for their children. But children sometimes persist in their misbehavior despite their parents' attempts to forestall them. At this juncture, parents may turn to physical punishment.

The conditions under which they punish their children physically, or refrain from doing so, appear to be quite different for the two social classes. *Working-class parents are more likely to respond in terms of the immediate consequences of the child's actions, middle-class parents in terms of their interpretation of the child's intent in acting as he does.*¹⁰ This should not be interpreted to imply that while middle-class parents act on the basis of long-range goals for their children's development, working-class parents do not. On the contrary, we believe that parents of *both* social classes act on the basis of long-range goals—but that the goals are quite different.

In an earlier study we have examined the relation of social class to the values parents most wish to see incorporated into their children's behavior.¹¹ We concluded that parents are most likely to accord high priority to those values which seem both important, in the sense that failure to achieve them would affect the child's future adversely, and problematic, in the sense that they are difficult to achieve. For working-class parents, the "important but problematic"

¹⁰ This distinction, of course, is closely akin to that made by Piaget in his discussion of "moral realism." See Jean Piaget, *The Moral Judgment of the Child*, Glencoe, Ill.: Free Press, n.d., pp. 104-194.

¹¹ Melvin L. Kohn, "Social Class and Parental Values," *American Journal of Sociology*, 64 (January, 1959), pp. 337-351.

⁹ In most instances the number of cases is so small as to preclude the possibility of statistical confirmation.

centers around qualities that assure *respectability*; for middle-class parents, it centers around *internalized standards of conduct*. In the first instance, desirable behavior consists essentially of not violating proscriptions; in the second, of acting according to the dictates of one's own principles. Here the act becomes less important than the actor's intent.

We believe that the reactions of parents of both social classes to their children's undesired behavior are entirely appropriate to their values. To say that working-class parents are particularly responsive to consequences and relatively unconcerned about intent, is equivalent to saying that their efforts are directed to enjoining disobedient, disreputable acts. To say that middle-class parents are more concerned about intent is equivalent to saying that their efforts are directed to encouraging their children to develop internalized standards and to act on the basis of these standards rather than externally imposed rules.

To see parents' reactions to their children's misbehavior as a function of their values helps to answer several questions which otherwise may be perplexing.

First, why are working-class parents so much more likely to punish their children physically for fighting than for arguing with their brothers or sisters?—or for aggressively wild play than for boisterousness? The answer seems to be that disreputability is defined in terms of consequences: the measure of disreputability is the degree to which the act transgresses rules of propriety. Fighting and wild play are disobedient, disreputable behaviors only when sufficiently extreme to be seen as transgressions of rules.

Second, why are working-class parents more likely to administer physical punishment when their daughters fight with friends, swipe something, or defiantly refuse to do as they are told than when their sons act in these ways? The answer seems to lie in different conceptions of what is right and proper for boys and for girls. What may be taken as acceptable behavior (perhaps even as an assertion of manliness) in a pre-adolescent boy may be thought thoroughly unlady-like in a young girl. Working-class parents differentiate quite clearly between the qualities they regard as desirable for their daughters (happiness, manners, neatness, and cleanli-

ness) and those they hold out for their sons (dependability, being a good student, and ambition). They want their daughters to be "little ladies" (a term that kept recurring in the interviews) and their sons to be manly. This being the case, the criteria of disobedience are necessarily different for boys and for girls. Obedience is valued highly for both. But working-class mothers who value obedience most highly punish their daughters physically for refusing to carry out parental requests and orders, while they are much less likely to take any action when their sons do so.

Middle-class parents make little or no distinction between what they regard as desirable for boys and for girls—the issue for both sexes is whether or not the child acts in accord with internalized principles. Therefore, the conduct of both boys and girls should be judged by the same criterion: intent. Our evidence supports this interpretation.

Finally, why do middle-class parents react so differently to aggressively wild play and to outbursts of temper? Why do they interpret these overtly similar behaviors as implying radically different intent? The answer is provided by the fundamental importance they attach to internal standards for governing one's relationships with other people and, in the final analysis, with one's self.

Wild play, however extreme, does not necessarily represent a loss of self-control, although it may indicate that the parent has lost control over the child. It may be regarded as a childish form of emotional expression—unpleasant, but bearable, since there are virtues in allowing its free expression in a ten- or eleven-year old. This is evidenced by the fact that those middle-class mothers who accord highest priority in their scheme of values to their children's happiness are least likely to punish wild play. An outburst of temper, however, may signal serious difficulty in the child's efforts at self-mastery; it is the type of behavior most likely to distress the parent who has tried to inculcate in his child the virtue of maintaining self-control. Again, the evidence supports the interpretation: those parents who value self-control most highly are most likely to punish their children for loss of temper.

If middle-class parents are to act in accord with their values, they must take explicit account of subjective and emotional factors, including the possible effects of punishment. They give considerable evidence that they do so. For example, when asked if there are any ways in which they would prefer to act differently toward the child, they are likely to cite the desirability of fuller understanding. When the child does poorly in school, they often try to be supportive, while working-class parents are likely to respond negatively. Of course, parents can rationalize. It is easy to believe that behavior which is at the moment infuriating ought to be punished. One gains the impression, however, that although middle-class parents may punish when angry, they try to restrain themselves—as they apparently do when they

believe their children's actions to be wild play.¹²

The working-class orientation, on the other hand, excludes or minimizes considerations of subjective intent, and places few restraints on the impulse to punish the child when his behavior is out of bounds. Instead, it provides a positive rationale for punishing the child in precisely those circumstances when one might most like to do so.

¹² It is revealing that the middle-class mothers who are most apt to report the frequent use of physical punishment are those who have least confidence in their ability to assess the child's intent—that is, the mothers who say they are “unsure of themselves” in dealing with their children. The working-class mothers who are most likely to make frequent use of physical punishment are those who are most sensitive to the immediate situation—who say they are “easily upset” by their children's actions.

A COMPARISON OF THE SEX TEMPERAMENT OF ALCOHOLICS AND MODERATE DRINKERS *

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A psychometric comparison of the sex temperament of 50 alcoholics and 50 moderate drinkers reveals that the former group has a significantly lower degree of masculinity than the latter. The role of deficient masculinity and/or latent homosexuality in the etiology of alcoholism is discussed within the framework of role and self theory. The dynamics of addiction are seen to operate as a special case of the general theory which ascribes decisive significance in personality disturbance to the discrepancy between personal characteristics and role expectations. Among the alcoholics, mother preference, broken marriages, odd-numbered ordinal positions, and possibly broken homes are associated with low degrees of masculinity. Both alcoholics and moderate drinkers with a preponderance of male siblings seem to show lower degrees of masculinity than do those with the opposite sibling distribution.

SEXUAL deviation as a factor in alcoholism has been noted by numerous investigators during the past forty years. The wide range of sexual aberrations reported by various writers to be present, in varying degrees, includes the following: disturbances of an oral character (oral eroticism, oral frustration, oral regression);¹

oedipal tendency (mother fixation, mother preference, mother attachment, mother in-

* The assistance of Leiper M. Robinson in the administration and scoring of tests is gratefully acknowledged. The cooperation of the staff of the Governor Bacon Health Center is also much appreciated.

¹ E. Bergler, “The Psychological Interrelation Between Alcoholism and Genital Sexuality,” *Journal of Criminal Psychopathology*, 4 (July, 1942), pp.

1-13; E. Bergler, “Contributions to the Psychogenesis of Alcohol Addiction,” *Quarterly Journal of Studies on Alcohol*, 5 (December, 1944-45), pp. 434-449; Ruth Bochner and Florence Halpern, *The Clinical Application of the Rorschach Test*, New York: Grune and Stratton, 1945, pp. 286-299; D. L. Gerard, “Intoxication and Addiction: Psychiatric Observations on Alcoholism and Opiate Drug Addiction,” *Quarterly Journal of Studies on Alcohol*, 16 (December, 1955), pp. 681-699; George Lolli, “Alcoholism and Homosexuality in Tennessee Williams’ ‘Cat On a Hot Tin Roof,’” *Quarterly Journal of Studies on Alcohol*, 17 (September, 1956), pp. 543-553; Paul Schilder, “Psychogenesis of Alcoholism,” *Quarterly Journal of Studies on Alcohol*, 2 (September, 1941), pp. 277-292.

dulgence, over protection);² narcissism;³ passivity, dependence, infantilism;⁴ sadism and masochism;⁵ "inadequate heterosexuality" (weak sex drive, impotence, incomplete sex structuring, "latent heterosexuality");⁶ compensatory heterosexual

activity ("sexual athleticism," "Don Juanism");⁷ latent or repressed homosexuality (and in some instances active or true homosexuality).⁸

These several kinds of deviation, of course, are not mutually exclusive classes. Nor do they usually appear singly in a given alcoholic personality; two or more may often form a part of the total syndrome of the case. Moreover, there is a lack of agreement regarding the generality, necessity, and sufficiency of sex abnormality as a factor in alcoholism. This problem is considered more fully below.

One of the most persistent interpretations of alcoholism assigns a prominent etiological role to latent or repressed homosexuality. Psychoanalytic explanations are especially partial to this factor—although not with uniform emphasis. Clinical studies by psychia-

² M. C. Bonney, "Parents as Makers of Social Deviates," *Social Forces*, 20 (October, 1941), pp. 77-87; E. Davidoff and C. A. Whitaker, "Pre-psychotic Personality in Alcoholic Psychoses," *Psychiatric Quarterly*, 14 (January, 1940), pp. 103-120; J. K. Jackson and R. Connor, "Attitudes of the Parents of Alcoholics, Moderate Drinkers and Non-drinkers Toward Drinking," *Quarterly Journal of Studies on Alcohol*, 14 (December, 1953), pp. 596-613; R. P. Knight, "The Psychodynamics of Chronic Alcoholism," *Journal of Nervous and Mental Disease*, 86 (November, 1937), pp. 538-548; Jacob Levine, "The Sexual Adjustment of Alcoholics—A Clinical Study of a Selected Sample," *Quarterly Journal of Studies on Alcohol*, 16 (December, 1955), pp. 675-680; N. D. Lewis, "Personality Factors in Alcoholic Addiction," *Quarterly Journal of Studies on Alcohol*, 1 (June, 1940), pp. 21-44; Harriet Mowrer, "A Psychocultural Analysis of the Alcoholic," *American Sociological Review*, 5 (August, 1940), pp. 546-557; E. E. Mueller, "Personality and Social Implications in the Life of the Alcoholic Veteran," *Quarterly Journal of Studies on Alcohol*, 6 (June, 1945), pp. 36-44; L. R. Sillman, "Chronic Alcoholism," *Journal of Nervous and Mental Disease*, 107 (February, 1948), pp. 127-149; E. Simmel, "Alcoholism and Addiction," *Psychoanalytic Quarterly*, 17 (January, 1948), pp. 6-31; E. A. Strecker, *Their Mothers' Sons: The Psychiatrist Examines an American Problem*, Philadelphia: Lippincott, 1946, pp. 122-127; J. H. Wall, "A Study of Alcoholism in Men," *American Journal of Psychiatry*, 92 (May, 1936), pp. 1389-1401.

³ N. J. T. Bigelow, S. R. Lehman et al., "Personality in Alcoholic Disorders," *Psychiatric Quarterly*, 13 (October, 1939), pp. 732-740; S. Rosenman, "The Skid Row Alcoholic and the Negative Ego Image," *Quarterly Journal of Studies on Alcohol*, 16 (September, 1955), pp. 446-473; E. Simmel, *op. cit.*

⁴ A. D. Button, "The Psychodynamics of Alcoholism," *Quarterly Journal of Studies on Alcohol*, 17 (September, 1956), pp. 443-461; A. D. Button, "The Genesis and Development of Alcoholism: An Empirically Based Schema," *Quarterly Journal of Studies on Alcohol*, 17 (December, 1956), pp. 671-75; Gerard, *op. cit.*; A. Kaldegg, "Psychological Observations on a Group of Alcoholic Patients," *Quarterly Journal of Studies on Alcohol*, 17 (December, 1956), pp. 608-628; G. Lolli, "Alcohol Addiction: An Outline of the Processes Leading to This Condition and Treatment," *American Journal of Nursing*, 48 (August, 1948), pp. 505-507; Mowrer, *op. cit.*

⁵ Rosenman, *op. cit.*; Schilder, *op. cit.*

⁶ R. S. Banay, "Cultural Influences in Alcoholism," *Journal of Nervous and Mental Disease*, 102 (September, 1945), pp. 265-275; Bergler, *op. cit.*;

Button, *op. cit.*; Davidoff and Whitaker, *op. cit.*; P. J. Hampton, "A Descriptive Portrait of the Drinker," *Journal of Social Psychology*, 25 (February-May, 1947), pp. 69-132, 151-170; S. C. Karlan and C. Heller, "Chronic Alcoholism: Psychiatric and Rorschach Evaluation," *Journal of Clinical Psychopathology*, 8 (October, 1946), pp. 291-300; Levine, *op. cit.*; F. G. Norbury, "Some Mental Mechanisms in Alcoholism," *Journal of the American Medical Association*, 118 (January, 1942), pp. 25-28; E. A. Strecker, "Chronic Alcoholism: A Psychological Survey," *Quarterly Journal of Studies on Alcohol*, 2 (June, 1941), pp. 12-17; Wall, *op. cit.*

⁷ Gerard, *op. cit.*; Mowrer, *op. cit.*; Wall, *op. cit.*

⁸ K. Abraham, "The Psychological Relations Between Sexuality and Alcoholism," *International Journal of Psychoanalysis*, 7 (January, 1926), pp. 2-10; Bergler, *op. cit.*; Bigelow et al., *op. cit.*; Bochner and Halpern, *op. cit.*; Button, *op. cit.*; L. P. Clark, "A Psychological Study of Some Alcoholics," *Psychoanalytic Review*, 6 (July, 1919), pp. 268-295; Gerard, *op. cit.*; Halpern, *op. cit.*; Otto Juliusberger, "Psychology of Alcoholism," *Zentralblatt für Psychoanalyse*, 3 (1913), digested in Lewis, *op. cit.*; Kaldegg, *op. cit.*; Levine, *op. cit.*; L. H. Loeser, "The Sexual Psychopath in the Military Service: A Study of 270 Cases," *American Journal of Psychiatry*, 102 (July, 1945), pp. 92-101; Lolli, *op. cit.*; W. R. Miles, "Psychological Factors in Alcoholism," *Mental Hygiene*, 21 (October, 1937), pp. 529-548; C. S. Read, "The Psychopathology of Alcoholism and Some So-Called Alcoholic Psychoses," *Journal of Mental Science*, 66 (July, 1920), pp. 233-244; Rosenman, *op. cit.*; Schilder, *op. cit.*; R. Strauss, "Alcohol and the Homeless Man," *Quarterly Journal of Studies on Alcohol*, 7 (December, 1946), pp. 360-404; Wall, *op. cit.*; S. Weijl, "Theoretical and Practical Aspects of Psychoanalytic Therapy of Problem Drinkers," *Quarterly Journal of Studies on Alcohol*, 5 (September, 1944), pp. 200-211.

trists and psychologists also find or suggest its presence with unusual frequency. Faced with this plethora of studies, the almost complete absence of psychometric investigations supporting or negating these findings and interpretations is surprising. The writer has found only one significant study of this character, but its methodological adequacy is questionable.⁹

The present study, first, attempts to determine whether male alcoholics and moderate drinkers differ with respect to "sex temperament," defined in terms of performance on a psychometric test; second, considers the implications of the findings for the etiology of alcoholism; and, third, establishes, in so far as the data permit, some of the correlates or possible causes of the varying degrees of masculinity and femininity.

HYPOTHESES

1. *Male alcoholics will show a lower degree of masculinity than moderate drinkers.* This is the major hypothesis of the study. If latent homosexuality is an important factor in alcoholism, and if this disposition is reflected in sex temperament (interests and response tendencies), this hypothesis follows. Correlative hypotheses pertaining to factors associated with varying degrees of masculinity are also advanced. In each instance it is expected that the degree of masculinity of the cohorts from the moderate drinker group will be higher than in the corresponding cohorts from the alcoholic group.

2. *Alcoholics and moderate drinkers who profess a preference for the maternal parent will show a lower degree of masculinity than*

those who do not. It is assumed that mother preference will express some measure of feminine identification.

3. *Alcoholics whose marriages have been broken by divorce or separation will show a lower degree of masculinity than those whose marriages are unbroken.* This hypothesis expresses the contention of some psychoanalytic writers that deficient heterosexuality is a cause of marital discord. (The presence of only one broken marriage among the moderate drinkers precludes a test of the hypothesis for this group.)

4. *Alcoholics and moderate drinkers with a preponderance of older male siblings will show a higher degree of masculinity than those with a preponderance of older female siblings.* The supposition is that normal sex identification of the younger male is facilitated by a surplus of masculine sibling models and impeded by a surplus of older female siblings.

5. *Alcoholics and moderate drinkers from broken homes will differ in degree of masculinity from those from unbroken homes.* It is surmised that the absence of a parent during the early period of life interferes with the child's sex identification.

Other *ad hoc* findings are discussed below.

PROCEDURE

The Terman-Miles Masculinity-Femininity Test¹⁰ was administered to 50 male inmates of the alcoholic ward of the Governor Bacon Health Center of the State of Delaware and to a control group of 50 male moderate drinkers,¹¹ simultaneously matched by age and educational categories as indicated in Table 1.¹² Degrees of masculinity

* J. Botwinick and S. Machover, "A Psychometric Examination of Latent Homosexuality in Alcoholism," *Quarterly Journal of Studies on Alcohol*, 12 (June, 1951), pp. 268-272. These researchers test for latent homosexuality in alcoholics by using the Minnesota Multi-phasic Inventory and the Terman-Miles tests of masculinity-femininity with negative results. One may suspect, however, that their method of selecting a sample of 33 cases may exclude precisely those who would have earned low masculinity scores. Also, it is not certain that scores on the Terman-Miles test are properly compared with the test constructors' norms. Moreover, the researchers employ no control group of either moderate drinkers or abstainers.

¹⁰ See Lewis M. Terman and Catharine C. Miles, *Manual of Information and Directions for Use of Attitude-Interest Analysis Test (M-F Test)*, New York: McGraw-Hill, 1938, pp. 1-19; see also, by the same authors, *Sex and Personality*, New York: McGraw-Hill, 1936.

¹¹ A "moderate drinker" is defined here as one who consumes alcoholic beverages in some amount, considers his drinking moderate, and who is engaged in steady employment.

¹² Scores on the T-M test vary inversely with age and directly with education level. In the present study only four alcoholics and three moderate drinkers completed less than seventh grade—the level below which Terman and Miles question the test's applicability. It is doubtful, however, that a

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$$t(\text{age}) = .45; .7 > P > .6 \quad t(\text{education}) = .21; .9 > P > .8$$

Group	- 100 to - 49	- 50 to - 1	0 to 49	50 to 99	100 to 150	150 to 199	\bar{X}	σ
Alcoholics	1	12	26	9	2	0	23.9	36.9
Moderate Drinkers	1	6	11	22	9	1	59.2	46.3

t = 4.15; P < .001

TABLE 3. MASCULINITY-FEMININITY SCORES OF ALCOHOLICS AND MODERATE DRINKERS WHO PROFESS MOTHER PREFERENCE OR NO PREFERENCE

Group	Mother Preference				No Preference				t	P
	N	Per Cent	\bar{X}	σ	N	Per Cent	\bar{X}	σ		
Alcoholics	16	32	8.4	27.4	25	50	32.2	29.2	2.64	.02 > P > .01
Moderate Drinkers	17	34	69.2	35.4	13	26	45.2	56.4	1.3	.3 > P > .2

M-F scores by marital status. The 15 alcoholics who were married show a mean M-F score of 34.1, whereas that for the 22 who were divorced or separated is 12.2. The difference is significant to the following degree: $t = 2.26$; $.05 > P > .02$. (Ten single alcoholics show a markedly bimodal distribution with a mean score that differs insignificantly from that of the married group.) As noted above, there is only one broken marriage among the moderate drinkers.

M-F scores by sex distribution of siblings. The findings on this relationship are entirely contrary to expectations. In none of three situations—older brothers but no older sisters, excess of older brothers over older sisters, and only male older and younger siblings—do either the alcoholics or moderate drinkers show higher M-F scores than those with the opposite sibling patterns. Indeed, Table 4 suggests the interesting possibility that an obverse hypothesis should be considered. Although the differences shown here are not statistically significant, they present a completely consistent pattern. In all three comparisons, for both alcoholics and moderate drinkers, the mean M-F scores are higher for those with a preponderance of feminine siblings. In the first two instances the dif-

ferences are relatively larger among the alcoholics than among the moderate drinkers. Especially significant perhaps is the fact that the highest scores for both groups are shown by those having an equal number of brothers and sisters.

M-F scores and broken homes. Twenty-eight per cent of the alcoholics and 20 per cent of the moderate drinkers come from homes that were broken by death, divorce, desertion, or separation before they had reached 15 years of age. Table 5 compares the mean M-F scores of those from broken and unbroken homes. The mean score for the alcoholics from broken homes is approximately one-third as large as that for the group from unbroken homes, the difference approaching significance. While the difference in mean scores for the moderate drinkers runs in the opposite direction, it is not statistically significant.

M-F scores and ordinal position. No significant difference in M-F scores is shown by either category for high and low ordinal position. (Fifty-eight per cent of the alcoholics and 42 per cent of the moderate drinkers were youngest or next to youngest children. The balance had two or more younger siblings.) However, a seemingly curious pattern marks the alcoholics: those with low M-F

TABLE 4. MASCULINITY-FEMININITY SCORES OF ALCOHOLICS AND MODERATE DRINKERS BY SEX DISTRIBUTION OF SIBLINGS

Sibling Pattern	Alcoholics		Moderate Drinkers	
	N	\bar{X} M-F	N	\bar{X} M-F
Older Brothers—No Older Sisters	13	16.6	4	63.0
Older Sisters—No Older Brothers	6	33.8	8	64.1
Older Brothers > Older Sisters	16	19.1	13	57.2
Older Sisters > Older Brothers	14	31.4	10	62.1
Only Male Siblings	8	4.1	7	40.4
Only Female Siblings	10	19.0	9	62.1
All Brothers = All Sisters	9	37.8	7	91.0

TABLE 5. MASCULINITY-FEMININITY SCORES OF ALCOHOLICS AND MODERATE DRINKERS BY BROKEN AND UNBROKEN HOME

Home Status	Alcoholics				Moderate Drinkers			
	N	Per Cent	\bar{X} M-F	σ	N	Per Cent	\bar{X} M-F	σ
Broken Home	14	28.0	11.0	31.6	10	20.0	73.8	47.1
Unbroken Home	32	64.0	31.2	37.4	40	80.0	55.5	45.1
$t = 1.82; .1 > P > .05$					$t = 1.1; .3 > P > .2$			

scores occupy predominantly odd-numbered ordinal positions. Table 6 shows the pattern of odd-even positions of those persons in the upper and lower halves of the distribution of M-F scores. Twenty of the 25 lowest scoring alcoholics held odd-numbered ordinal positions in their families; the difference in the distributions is highly significant. The moderate drinkers show no such difference. Of comparable interest is the fact that 12 of the 16 alcoholics professing mother preference were odd-numbered children, a difference not shown by the mother-preferring moderate drinkers. We also note that the lowest mean M-F scores by ordinal rank for both alcoholics and moderate drinkers are shown by third-placed offspring.

M-F scores by age and education level. As indicated above, scores on the Terman-Miles test vary inversely with age and directly with educational level. The scores of the moderate drinkers in the present study clearly reflect this pattern of variance, but those of the alcoholics do not. The coefficients of correlation for M-F scores and age are $-.025$ for the alcoholics and $-.302$ for the moderate drinkers; those for M-F scores and educational level are $-.415$ for the alcoholics and $.299$ for the moderate drinkers.

DISCUSSION

The findings presented here ostensibly confirm the major hypothesis of this study and

support the conclusions of the numerous psychoanalytic and psychiatric investigations cited earlier. A number of problems, however, must be considered.

First, is latent homosexuality necessarily reflected in measures of sex temperament? Terman and Miles report that the scores made by homosexuals deviate toward the mean for the opposite sex. It is possible that the low scores of our alcoholic subjects reflect homosexuality. But Terman and Miles also warn against assuming that such deviant scores are an adequate basis for diagnosing either latent or overt homosexuality. It is possible, therefore, that our alcoholic subjects, although more feminine in temperament than our moderate drinkers, are not correspondingly more homosexual. The data provide no basis for choosing between these possibilities and thus are equally consistent with two theories: that alcoholism is a function of homosexuality, and that alcoholism is a function of femininity of temperament. The data are also consistent with the proposition that alcoholism is a function of homosexuality *or* femininity of temperament *or* both. The dynamics leading to compulsive drinking can be formulated in such a way that homosexuality and femininity of temperament become functionally equivalent, playing the same role in the addictive process. Such a formulation is consistent with role "theory" and "theory" of the self.

In any event, the essential factors in the

TABLE 6. "ODD-EVEN" ORDINAL POSITION OF ALCOHOLICS AND MODERATE DRINKERS IN UPPER AND LOWER HALVES OF M-F DISTRIBUTION

Sector of M-F Distribution	Alcoholics		Moderate Drinkers	
	Odd	Even	Odd	Even
Upper Half	10	15	17	8
Lower Half	20	5	15	10
$\chi^2 = 8.33; P < .005$			$\chi^2 = .007; .975 > P > .95$	

dynamics of addiction appear to include tension, conflict, and associated reactions, the discovery of the pharmacological capacity of alcohol to relieve tensions and insecurities, the availability of alcohol, and the social tolerance of its consumption. The psychodynamic sequence is suggested by Timm: "A [subconscious] urge . . . which is repugnant to the ego . . . is repressed. The urge . . . carries a high emotional overload and when denied expression in consciousness, provides tension. This demands relief. The individual is not sufficiently integrated to sublimate the effect. . . . He takes alcohol. . . ." ¹⁴ Characterizing the latent homosexuality hypothesis as an approximation of a more valid generalization, Gerard advances a more sociologically oriented view applicable to both the alcoholic and opiate addict: "A more accurate generalization may be that the alcoholic and the opiate addict both are persons who have failed in varying degree to develop a definite sexual role or identification. Among the special cases of this indefiniteness of role and identification is an orientation to homosexual closeness which, because it is unacceptable for the adult male in our culture, is often perceived with great anxiousness." ¹⁵ The consequence for the "special cases" of homosexual disposition seem to be essentially the same for men with a low masculinity of temperament. In this case, role conflict results perhaps from the inability or reluctance of such persons to meet the norms or social expectations set for the masculine role in our culture. For example, the individual with the traits of passivity, dependency, and the like, more appropriate to a feminine role, is subject to expectations and pressures to display more aggressive, independent, masculine behavior—or to perceive himself as deviant. The consequent tensions and anxieties seem sufficient in many cases to lead to pathological modes of relief. In this perspective, both the homosexual and the feminine temperament explanations may be seen as special cases of the more general theory which attributes decisive significance to the discrepancy between personal

characteristics and expectations attached to the roles with which the individual is identified by himself and others.

Of course, homosexuality and femininity of temperament *as such* are not significant for this general theory, but only insofar as they tend to produce the kind of discrepancy described above. Whether or not these dispositions will be incompatible with role expectations depends upon how masculinity is defined and upon the nature of the corresponding expectations in the actor's reference groups. There is reason to believe that such definitions and expectations may vary considerably in different sectors of the social system and in different sub-cultures. Thus Terman and Miles report widely varying mean M-F scores for occupational groups, for example, 94.3 for engineers, 27.9 for policemen and firemen, and 8.4 for artists. ¹⁶ (The mean score for alcoholics in the present study is 23.9). It is quite possible that low scores do not signify temperaments and personalities which are inconsistent with the internalized role expectations and the perceived expectations of others. This suggests the possibility of research in which the alcoholism variable is correlated, not with absolute scores, but with deviations from the mean scores of the subjects' occupational groups or other presumptive reference groups.

In any case, no exclusive relationship holds between sexual deviation and alcoholism. This is not to deny the high frequency of their co-existence. There are undoubtedly many alcoholics whose affliction was caused, at least initially, by disturbances other than sexual. Likewise there are probably many sexually maladjusted individuals whose disabilities lead them to other forms of compulsive or escapist behavior, such as drug addiction, obsessive gambling, or food addiction. Simply put, unresolved maladjustments arising from various sources can lead to various forms of abnormal behavior. The course which individual cases follow depends upon differential association, possibly differential organic make-up, the role symbolism of drinking, and the social acceptability of drinking as a solution to problems of adjustment in the subject's groups.

¹⁴ O. K. Timm, "Psychodynamics of Alcoholism and Its Relation to Therapy," *Medical Bulletin*, Washington, D. C.: Veteran's Administration, 20 (July, 1943), pp. 42-48.

¹⁵ Gerard, *op. cit.*, p. 692.

¹⁶ Terman and Miles, *Manual of Information* . . . , *op. cit.*, p. 159.

The final comment in this context poses a question for further study: Is moderate drinking correlated with above-average masculinity? The mean M-F score of our moderate drinkers is considerably higher than that of the males of comparable age and education studied by Terman and Miles. Perhaps controlled drinking, in contrast with abstinence or compulsive drinking, is a mark of masculinity in our culture.

The balance of the discussion briefly deals with some of the factors shown to be associated with the main variable.

One of the most ubiquitous factors found in the alcoholic's background is a seemingly abnormal relationship with the mother. Mother attachment, preference, fixation, or indulgence, is reported in some 15 studies listed in footnote 2. Estimates of its frequency vary from about 35 per cent (Davidoff and Whitaker) to about 80 per cent (Mueller and Strecker). This kind of relationship with the mother is commonly paralleled by a father relationship marked by fear, hostility, or indifference. Levine observes that, for the alcoholic, "There is rarely a healthy male figure with whom identification is possible. . . ." ¹⁷

Our prediction that drinkers professing a mother preference would show a lower degree of masculinity is confirmed for the alcoholics but not for the moderate drinkers. Although the frequency of mother preference is about the same in the two categories, the meaning of the relationship appears to be different. For alcoholics, lacking adequate fatherhood, mother preference may lead to a strong feminine identification and dependency expressed in low masculinity and, perhaps, passive or latent homosexuality. For moderate drinkers, however, mother preference may express a relationship marked by no pathological substructure and, indeed, one which is socially approved. Supported by a "healthy" father image, mother preference may possibly elicit a high degree of masculinity in a manner suggested in the subsequent discussion of the sibling pattern.

The close relationship between alcoholism and marital discord is well documented. Of course, the nature of the relationship is variable. Alcoholism may be a cause or an

effect of discord, the two may operate reciprocally, or both may be symptoms or effects of the same underlying factors. One of these factors may be latent homosexuality or low masculinity. The hypothesis that alcoholics whose marriages were broken would show lower M-F scores than those with intact marriages seems to be clearly confirmed by the data. At least two kinds of dynamics could be operating here. Deficient heterosexuality may inhibit satisfactory sex adjustment and thus impair the marital relationship. Or the alcoholic's weak masculinity may impede the performance of an effective male role as husband (and father) and so hinder the fulfillment of the wife's needs and expectations. In either event a high frequency of discord is predictable. In this connection, recall that our moderate drinkers, with evidently a higher than average mean M-F score, report only one broken marriage.

The unexpected relationship between M-F scores and sibling pattern, reported above, negates our hypothesis and poses a question concerning the way in which sex identification develops. Some writers who deal with this process apparently assume that older siblings of the same sex facilitate normal identification in the younger child whereas older sibling of the opposite sex interfere with such identification.¹⁸ Our findings suggest, however, that within the family *two* factors are at work in the child's acquisition of the temperament and behavior appropriate to the given sex role. The first is the presence of one or more models of the same sex who articulate norms that the younger sibling is induced to imitate. The other factor may be called the *model-complement*, which is made available by the presence of one or more older siblings of the opposite sex. The young boy, for example, acquires masculinity not only through incorporating the norms of a male model but also through clearer perception of the norms of the opposite or complementary female role. Moreover, the model comple-

¹⁸ See, e.g., H. L. Koch, *Attitudes of Young Children Toward Their Peers As Related to Certain Characteristics of Their Siblings*, *Psychological Monographs*, No. 426, 1956, pp. 29, 33. It should be noted, however, that Terman and Miles (*op. cit.*, p. 228) "find a tendency for men with siblings of one sex only to be more masculine if the siblings are sisters rather than brothers." They refer to this as a "contrast effect."

¹⁷ Levine, *op. cit.*, p. 680.

ment enables him to define his role in contrast to the perceived role of the opposite sex (that is, it provides a model of what one's role is not), and it provides an opportunity to play and to practice one's role *vis-a-vis* his counter-role. (The process operates, of course, in a comparable manner in the child's interaction with the mother and father). Any generalization concerning the relative strength of these two influences is probably unjustified. It is in point, however, to recall that the highest degree of masculinity in both of our categories is shown by those having an equal number of brothers and sisters.

In advancing our hypotheses, it was surmised that the loss of one or both parents by mid-adolescence has a disturbing effect upon the child's sex identification or temperament. Such deviation, however, does not appear in either of our categories to a measurably significant extent. But the direction of the difference in scores between the two categories shown in Table 5 prompts the suggestion that further study of broken homes, as in the case of mother preference, would show obverse relationships between M-F scores of alcoholic and moderate drinkers. This speculation can not be developed here.

The sketchy data on the family relationships of the alcoholics do not permit an explanation of the concentration of low-scoring individuals in the odd-numbered ordinal positions. Nor is speculation about this finding possible here. The distribution shows no relationship to sibling patterns.

Finally, we are unable to account for the fact that the correlations between M-F scores and age and education for moderate drinkers conform to the norm pattern of the T-M test, whereas those for alcoholics do not. The lack of correlation between M-F scores and age for alcoholics, however, suggests that prolonged excessive drinking is not a cause of low masculinity or deviant sexuality. If it were, we should expect to find an inverse correlation indicating a more than normal decline of M-F scores as age increases.

SUMMARY

Numerous psychoanalytic, psychiatric, and psychological studies specify deviant sexuality as a common factor in alcoholism. This investigation of 50 male alcoholics and 50 moderate drinkers seeks to test for the presence of latent homosexuality or low masculinity among alcoholics. Certain correlates of sex temperament are also examined.

1. The alcoholics in this study show a significantly lower degree of masculinity than do the moderate drinkers. The question of the relationship between low masculinity of temperament and latent homosexuality is considered but not conclusively answered.

2. The alcoholics who profess a preference for the mother show a significantly lower degree of masculinity than do those who express no preference. The moderate drinkers fail to show a comparable difference in this respect.

3. The alcoholics whose marriages were broken show a significantly lower degree of masculinity than do those whose marriages were intact. Only one moderate drinker reported a broken marriage.

4. Neither alcoholics nor moderate drinkers with a preponderance of older male siblings, or with exclusively male siblings, show a higher degree of masculinity than those having the opposite sibling patterns.

5. The findings suggest that alcoholics who come from broken homes have a lower degree of masculinity than those who do not.

6. Alcoholics scoring low on the masculinity scale, as well as alcoholics with a mother preference, occupy predominantly odd-numbered ordinal positions. Moderate drinkers show no difference in this respect.

7. Whereas the masculinity-femininity scores of the moderate drinkers vary with age and education level in accord with the norms of the test used, those of the alcoholics do not.

CRUELTY, DIGNITY, AND DETERMINISM

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Resolution of controversy about "free will" and consequent "moral responsibility" requires that questions about these subjects be phrased as propositions amenable to verification. Translation of such questions into verifiable form permits testing for the consistency of assumption used in the explanation and control of behavior and for the alleged effects of belief in these assumptions. One such test is reported of such consequences. Indeterminists have argued that the accordance of "dignity" to man requires this assumption. Where "human dignity" is defined in a fashion that does not beg the question of free will—that is, non-tautologically as "resistance to revenge and the infliction of pain"—it is found that dignity is more likely to be accorded humans by determinists than by indeterminists. Therefore, other studies of the alleged consequences of the ascription of "free will" and "moral responsibility" may show these ideas to be irrelevant, if not inimical, to the achievement of commonly professed social goals.

ARGUMENTS about free will and determinism have much of the quality of children yelling "'tis," "'tain't." The difficulty arises, in part, from the form of the question, "Does man have free will and, consequently, moral responsibility, or not?" Put this way the question is probably irresolvable and it is to be expected that the debate will go off on all the usual polemic tangents of anecdote and analogy, *ad hominem*, and dire prediction unless. . . .

It is more fruitful to discuss this ancient problem by asking the question in a manner that allows for empirical test. Ladd¹ has suggested that "the problem of 'freedom of the will' is primarily an empirical problem . . .," and certainly it should be so viewed if the issues are to be resolved. But to make the problem empirical demands, to begin with, that "clear and answerable" questions be asked,²—and this requires that we know when we are offering a definition and when we are stating a proposition. The confusion of analytic with synthetic statements persists and leads men to believe that they have affirmed something about a subject when they have told us only how they are using a word. "You won't get bald if you don't lose your hair" looks like information while it predicates nothing. Similarly, in the debate about determinism, it is not unusual

to hear a definition propounded as if it were a proposition:

Without free will man lacks moral responsibility and, hence, dignity.

What are these?

Human dignity and moral responsibility lie precisely in the ability, peculiar to man as opposed to dogs, apes, insects, and things, to make rational choices, and it is these rational choices which we call free.

Such statements affirm little about man and his world, but they do tell us how their maker is using words.

If we are to treat the problem empirically, it must be recognized that free will and responsibility are not qualities that a man *has* or doesn't *have*. Nor are they things that he *is*.³ Free will and responsibility are attributes one assigns to himself and others under certain circumstances, for certain reasons, stated and unstated. The only meaningful question concerns the usefulness of this assignment. And this is a question of determinable consequences. We are now on empirical ground.

On this *terra firma*, it becomes possible to test ourselves for the consistency of our assumptions and, where inconsistency is discovered, to examine whether we may drop the useless baggage of contradictory premises.

¹ John Ladd, "Free Will and Voluntary Action," *Philosophy and Phenomenological Research*, 12 (March, 1952), pp. 392-405.

² G. A. Lundberg, C. C. Schrag, and O. N. Larson, *Sociology*, New York: Harper, 1954, p. 7.

³ "Have" and "is" remain troublesome words which would be out of place to explore here. The point is to avoid hypostatization which, because of the emotions involved, becomes a more difficult task with words like "free" and "responsible" than with less glorious descriptors.

The mental luggage one must cart about if he is to remain an indeterminist in a determined world is a heavy freight. Most men solve this problem by behaving like Russell's model: they don't believe in free will until they think about it.⁴

In the Western World determinism is a cab everyone rides when he wants to go anywhere—other than the destination of a moral judgment. Then, suddenly, and usually unconsciously, conveyances are shifted and less mundane means—acts of “free” choice—are required to arrive at rectitude. In most men the inconsistency is swallowed whole; others face up to the contradiction but feel that the opposed assumptions are necessary, like Plato's myths for the masses and Father Day's religion. One writer tells us that “we live in the same deterministic world as we did B.Q. (Before Quantum) or we could not build a bridge or the simplest machine. And man is part of this deterministic world. It must still be said that free will is an illusion that must be treated as reality.”⁵ This at least is frank, but is it necessary? Hall advises: “Given the scientific purpose to understand conduct, determinism is a necessary postulate. Given the additional purpose to evaluate conduct, some degree of autonomy is a necessary postulate.”⁶ Here is suggested a principle of *moral complementarity*: for understanding and controlling behavior, assume it to be “caused;” for moral judgment of behavior, assume the opposite. But having thus separated assumptions, it is possible to test whether they are in fact necessary to the objectives at hand and, as in the history of all science, the door is open to simpler and more consistent assumptions. It is possible, for example, to question Hall's second proposition that the evaluation of conduct requires the assumption that the behavior is undetermined.

To recapitulate, it has been argued that the “free will” problem can best be resolved

when the questions asked are made empirical, that to make them empirical requires the recognition of the distinction between a proposition and a tautology since it is only the former for which we can profitably test in experience, and that once the questions have been so winnowed we stand in a position to examine our premises for consistency and to inquire of their consequences.

It is contended here that, first, modern protagonists of free will—of which there are many recent spokesmen from journalism, law, physics, and psychology⁷—are inconsistent; second, many of the declared consequences of the belief in indeterminism are definitions, not propositions; and, finally, when the alleged consequences are put in verifiable form, their dependence upon belief in “free will” and “moral responsibility” is questionable.

Since the putative benefits of indeterminist faith are many, one short paper can not answer all allegations. It is my intention, rather, to raise certain questions that make some aspects of the “free will” problem amenable to empirical solution, and to provide a sample test of two facets of this problem.

A TEST: DEFINITIONS, HYPOTHESES, PROCEDURES

A clever debater's device, often noted among indeterminists, consists of associating one's argument with something good and, preferably, vague so that the opposition is placed in the uncomfortable position of seeming to recommend evil by the maintenance of its case. Thus we are told that free will is necessary if man is to be “morally responsible” and if the “dignity of man” is to be preserved. These are alleged consequences of a belief. Hall, for example, maintains that “if human being are in any degree free moral agents, then treatment cannot be wholly substituted for punishment; treating all criminals as *ipso facto* sick persons cannot be justified even on humanitarian grounds. A

⁴ “. . . free will is always unhesitatingly rejected except when people are thinking about the free will problem.” Bertrand Russell, *Human Society in Ethics and Politics*, New York: Simon and Schuster, 1955, p. 79.

⁵ J. M. Martinez, “Letters,” *Saturday Review*, 41 (April 26, 1958), p. 21.

⁶ Jerome Hall, “Psychiatry and Criminal Responsibility,” *The Yale Law Journal*, 65 (May, 1956), pp. 765-766.

⁷ See, e.g., E. A. Mowrer, “The Open Universe,” *Saturday Review*, 41 (April 19, 1958), pp. 11-13, 43-45; A. H. Compton, “Science and Man's Freedom,” *Atlantic*, 200 (October, 1957), pp. 71-74; E. Boring, “When is Human Behavior Predetermined?” *Scientific Monthly*, 84 (April, 1957), pp. 189-196; Hall, *op. cit.*

dogma that equates normal adults with helpless victims of disease is incompatible with respect for personality and distinctive human traits."⁸

Now if one accepts this statement as providing definitions, there can be no quarrel, but there is danger that such definitions masquerade as information. As ordinarily used, the terms "free moral agent," "human dignity," and "respect for personality" are tautological. But when the key concepts are not defined in terms of themselves, when they are phrased so as to express the empirical consequences of a belief, the indeterminist is shown not only to be using inconsistent assumptions—a fact he sometimes admits—but to be achieving thereby less, if not the opposite, of what he says he intends. The data presented below demonstrate, without becoming *echt* Freudian, that the advocate of free will doesn't *really* believe in what he says he intends, or that he *really* intends something other than what he tells us and himself.

The case in point concerns the consequence of belief in free will for "human dignity" and "respect for personality." Respect for personality and the accord of dignity to man may be defined in other than free will terms as resistance to vengeance. We may say that *those individuals have most "respect for human personality and human dignity" who, among other things, are most resistant to revenge and who are loathe to inflict pain.*

This definition, like all definitions, is arbitrary. It expresses an interest in attending to something rather than everything. It has justification in the community of ideas usually associated with these terms; and determinists and indeterminists alike can agree on it (at least indeterminists, I believe, would be hard placed to advocate harming man as a means of dignifying him). Further, it has an advantage over the definition that assigns dignity only to organisms that can make "uncaused choices," in that the *definiendum* in such a definition begs the question whereas in the definition proposed the *definiendum*, the proclivity to punish, is observable and independent of the *definiens*.

If, then, it can be agreed to let "respect for the dignity of man" stand for a non-

punitive attitude toward him and, *a fortiori*, a non-punitive attitude in the face of his deviation from social standards, then one facet of the free will problem has been made susceptible to empirical test.

It is hypothesized that those who, when thinking about free will plump for it, are the kinds of people who are more apt than others to deny dignity to man. By this is meant that they are more apt to hurt people or to want to see them hurt—and to want them hurt in a disinterested fashion which means, as Ranulf puts it, that "no personal advantage is achieved by the act of punishing. . . ."⁹

This desire for disinterested revenge is held to vary among societies and among classes within a society.¹⁰ As corollaries of the primary hypothesis, it is proposed that such desire will be found in association with authoritarianism, racial prejudice and discrimination, envy, low empathy, and a generalized lack of love, insight, and sympathy. In short, it is contended that belief in man's free agency, blameworthiness, and responsibility go with the denigrating attitudes cited above. These attitudes, then, are suggested as additional definitions of indignity to man.

Neither the primary hypothesis nor its corollaries says anything about the wisdom of punishment or its efficacy in achieving certain objectives with certain individuals under specified conditions. It is predicted merely that indeterminists will be crueler than determinists, and crueler in a disinterested way. But it is proposed, as a further subsidiary hypothesis only to be touched upon here, that indeterminists are less concerned with the consequences of punishment than they are with its execution. This motive, it is claimed, distinguishes them from determinists who do not disapprove of punishment categorically, but who wish to know the circumstances under which punishment and reward will effect the goals sought.

An opportunity to probe the consistency and cruelty of indeterminists arose through the polling by anonymous questionnaire of 939 "community leaders" in one of the country's ten or twelve largest metropolitan

⁸ Svend Ranulf, *Moral Indignation and Middle Class Psychology*, Copenhagen: Levin and Munksgaard, 1938, p. 1.

¹⁰ *Ibid.*

^{*} Hall, *op. cit.*, pp. 765-766.

areas. The poll was designed to provide a picture of how these citizens defined certain types of children's problems.¹¹ Respondents were selected because of their involvement, either professionally or as board members of child welfare agencies, in determining what the community does for its children. Therefore, they are not representative of the entire city ethnically or occupationally. It is probable that the underrepresentation of the lower and, particularly, lower-middle classes has reduced the number of extreme punitive scores,¹² as has the context of the administration of the questionnaire. This possible restriction of range means, of course, that any association discovered between indeterminism and punitiveness is probably lower than that which obtains in the population at large. The 939 subjects include the following:

Welfare Agency Boards and Committees (composed principally of business executives, lawyers, and other professionals, and upper-status housewives)	299
Child Welfare Agency Professional Staffs	195
Educators (public, private, and parochial administrators, teachers, counsellors, and school social workers)	358
Ministers (returns from a one-third sample of religious leaders in the county)	49
Pediatricians (returns from a one-half sample of diplomates in the county)	32
Chiefs of Police and Heads of Juvenile Police Bureaus	6

The questionnaire, administered at group meetings and through the mail, includes seven items which tap opinions about the determination of behavior. Their content concerns degree of belief in the formation of personality through what has happened to an individual, his ability through "will" to change himself, and his "responsibility." For 100 of these subjects selected at random five items form a unidimensional scale with a reproducibility coefficient of 90 per cent by Edwards' method,¹³ a minimal marginal re-

producibility of 77 per cent, and yielding six scale types when scored dichotomously. The items with their original response categories are listed in Figure 1. Item numbers refer to their order in the questionnaire. From its content, it seems fair to call this scale a measure of "determinism."

Derivation of this scale substantiates Russell's comment that everyone is a determinist until he thinks about it. The thoughtway underlying majority opinion may be paraphrased: "All behavior is caused but if a man 'really' wants to he can overcome the causes that beset him—or at least he can if he has his wits about him since intelligence, remember, is 'free'." This seems to describe the thinking that allows 92 per cent of these respondents to affirm that a juvenile delinquent "gives signs before he gets in serious trouble," 86 per cent to agree that "the early years of life from birth to age seven form the basic personality we see in the adolescent and adult," and 64 per cent to assent to the probability of neurotic adults having been neurotic children. But as the questions move from behavior to ideology, indeterminism and submission to the favored free-will words show themselves. Fifty-seven per cent believe that a person can "straighten himself out" if he "really wants to," and 87 per cent agree that a 17-year-old person is "responsible."

In holding that this simple test lends credence to Russell's position, nothing is said about which assumption, determinist or indeterminist, is the more fruitful. All that is indicated is that people don't believe in free will except when they are asked to think about it. That this kind of thinking is also to be found among scholars attests only to the extent of an ideology, not to its usefulness or consistency.

Cruelty (punitiveness) is measured by a four-item scale which yields an Edwards-type reproducibility coefficient of 90 per cent, a minimal marginal reproducibility of 71.8 per cent, and five scale types under dichotomous scoring (Figure 2). It should be noted that these items allow the expres-

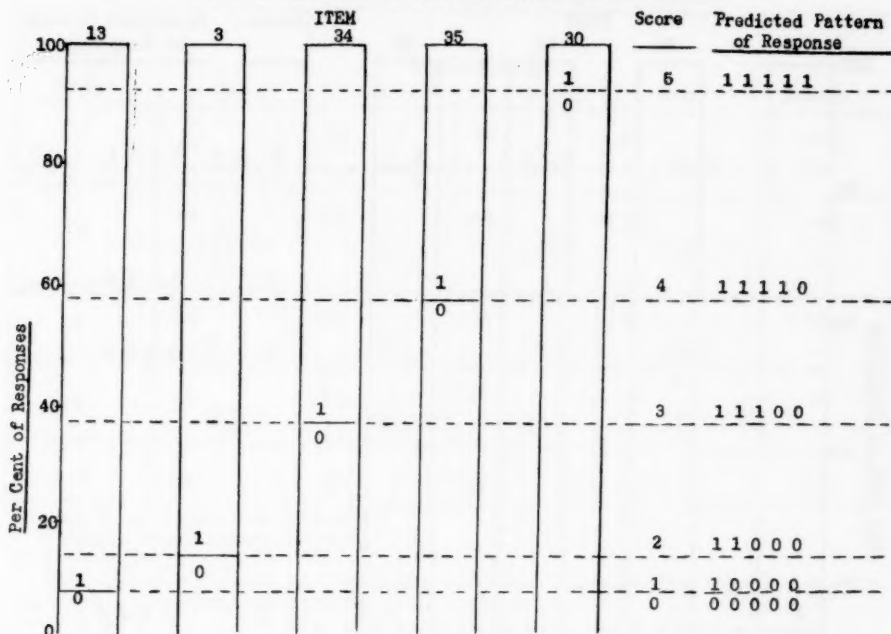
¹¹ These data derive from one project of the Child Welfare Study of the Community Council of Houston, subsidized in largest part by the Hogg Foundation for Mental Health and the W. B. Sharp Foundation. The opinions and hypotheses expressed in this paper are the writer's, of course, and should not be attributed to any organizations with which he is directly or indirectly associated.

¹² Cf. Ranulf, *op. cit.*

¹³ As opposed to the Cornell scaling technique, Edwards' method tends to give lower, but more accurate, coefficients of reproducibility. Justification

for retaining items that split higher than 80-20 is found in the improvement in the reproducibility coefficient over the minimal marginal reproducibility. A. L. Edwards, *Techniques of Attitude Scale Construction*, New York: Appleton-Century-Crofts, 1957.

FIGURE 1. THE DETERMINISM SCALE (High Score—Deterministic)



13. Does a boy who becomes a delinquent such as described above * give signs of his tendency before he gets in serious trouble?

3. "The early years of life from birth to age seven form the basic personality we see in the adolescent and adult." Do you agree or disagree?

34. Do you believe that emotionally disturbed (neurotic) adults were also disturbed as children?

35. "If a person really wants to straighten out, he can solve most of his personal problems by himself." Do you agree or disagree?

30. Assuming that a boy or girl is not feeble-minded or mentally ill (psychotic), do you believe that by age 17 he is "responsible" for his actions, that is, is able to control his behavior in accordance with the community's laws and morals?

* Refers to Item #11: "Take the case of a 16-year-old white boy who has been brought to the attention of the police several times since he has been 14 years old for such behaviors as malicious mischief, theft, burglary, runaway, and fighting. . . ."

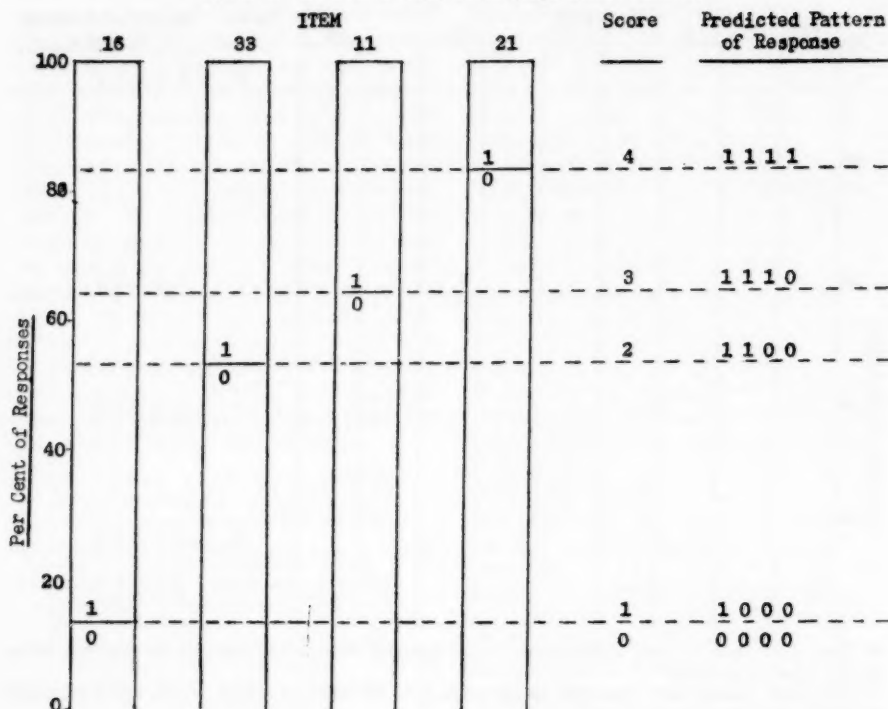
sion of that kind of disinterested punitiveness of which Ranulf speaks.

If the indeterminist is not a consistent believer in free will, the punishing man—and we shall see that they are cousins—is none too clear about the results of his punitive prescription. And this lends credence to one of the collateral hypotheses given above, that the indeterminist's concern may be less with results than with revenge. For, while most respondents recommend punishment, or some combination of punishment and treatment, in response to behavioral deviation, they have little faith in the rehabilitative efficacy of present punitive devices. Asked about the reformatory performance of

the state school for boys, 487 of the 910 (53.5 per cent) respondents felt that more boys repeated crimes after release than were reformed. An additional 21.5 per cent put the score at about 50–50, and another 21.3 per cent admitted their ignorance of the results. Only 33 of the 910 (3.6 per cent) respondents to this question believed that most boys were saved from further crime by their incarceration at G. . . .

Table 1 shows the distribution of punitive scores by determinism scores for the 778 subjects for whom they were available. This table gives a Chi-square of 69.43 which is significant much beyond the .001 level.

FIGURE 2. THE PUNITIVENESS SCALE (High Score—Punitive)



16. In general, do you regard such delinquency (refers to Item #11), after it has been apprehended, as requiring punishment or treatment?

33. And in such a case as #32 above,* what do you think would be the most effective way of handling such a child? —1. Through treatment procedures now known but not used here. —2. Treatment through presently available social agencies. —3. Doing what we do now in this community. —4. Punish him and then treat him. —5. Punish him more severely. —6. I don't know.

11. Take the case of a 16-year-old white boy who has been brought to the attention of the police several times since he has been 14 years old for such behaviors as malicious mischief, theft, burglary, runaway, and fighting. Please check the procedure you would recommend in such a case: —1. He should be sent to state prison. —2. He should be sent to an institution like "State Reform" until he is 21. —3. He should be sent to such an institution for as long as the judge decides. —4. He should be sent to an institution like "State Reform" indeterminate, i.e., until the superintendent and/or parole board there feels he is ready for release on parole. —5. He should be placed on probation. —6. He should be released to his parents. —7. He should be excluded from the county if there are relatives elsewhere he can live with. —8. He should be sent to a children's assessment center where he would be detained while given a complete medical, sociological, and psychiatric examination, and professional recommendations made for his care.

21. If there are 'problem children' who drop out of school or are suspended from school for long periods of time, what do you believe should be done about them? —1. Establish special schools for them—schools with a heavy vocational bent. —2. Provide a caseworker to help adjust their problems. —3. Force them to attend the regular schools by punishing them for non-obedience. —4. Nothing. —5. Other _____ (specify)

* Item #32 reads: "In the case of a child age 17 who was neither mentally ill nor feeble-minded and who seemed to know what is expected of him legally and morally but who persisted in behaving otherwise. . . ."

TABLE 1. THE RELATIONSHIP BETWEEN "DETERMINISM" AND "PUNITIVENESS"

PUNITIVE- NESS	DETERMINISM				
	0-1	2	3	4-5	Total
0	6 <u>6.5</u>	16 <u>29.5</u>	32 <u>57.4</u>	87 <u>46.6</u>	141
1	13 <u>18</u>	79 <u>82</u>	158 <u>159.5</u>	136 <u>129.5</u>	386
2	14 <u>9</u>	48 <u>41</u>	104 <u>79.8</u>	30 <u>64.8</u>	196
3	3 <u>2.5</u>	21 <u>11.5</u>	25 <u>22.3</u>	6 <u>18.1</u>	55
Total	36	164	319	259	778

High Scores—punitive and deterministic.

Underlined figures—expected frequencies.

$\chi^2=69.43$, $P<.001$.

Low expected frequency in cell "M" qualifies probability; significance and congruence of distribution remain with determinism scores 0-1 and 2 combined.

(With nine degrees of freedom, a Chi-square of 27.88 is to be expected by chance one time in a thousand.) The deviations from chance distribution are in the direction predicted by hypothesis: *indeterminists are more likely to recommend disinterested punishment in response to behavioral deviations*.¹⁴ If the definition here proposed is accepted—that such recommendation robs human personality of dignity and respect—one alleged consequence of the dogma of free will has been disproved, within the limits of the instruments and subjects used.

¹⁴The indeterminist is hoist on his advocacy of punishment for behavioral deviation for if he justifies his recommendation on the grounds that it has consequences—reform or deterrence—he has become a determinist, that is, he is assuming that behavior is an effect. But if he denies any such pragmatic basis for his prescription we have even greater cause to read other motives into his punitiveness, principally, the disinterested desire for revenge. And the fact that the punisher seems not to have confidence in the reformative effects of his recommendations lends credence to the possibility that he is merely vengeful, although the indeterminist may defend himself by arguing that, while he does not believe in the reformative value of punishment he does believe in its deterrent effect. Again, he has become a determinist.

SUMMARY AND STUDY PROPOSALS

It has been proposed that steps toward the resolution of the arguments hinging on free will and moral responsibility involve the rejection of definitions as propositions and the phrasing of the relevant questions in a manner susceptible of verification so that consistency of assumption and accuracy of predicted consequence may be tested. It has been shown that the "layman" is an inconsistent determinist, that, except when judging others (and himself?) on "moral" issues, he does not reason on the basis of the premises he otherwise professes. Further, a test of one of the alleged consequences of belief in "free will" does not support the indeterminist position.

Additional questions and tests are proposed as a means of resolving the "free will" issue:

1. *Must behavior be based on choice in order to be moral?*
2. *Must such choice be "free" in order to be moral?*

These questions ask only for definitions, not information. Yet thinning out the definitions, showing the consequences they imply and testing for them, may help us to discover

whether "what we really want" is aided or impeded by the thoughtways imbedded in them.

Assume that "what we really want"—determinist and indeterminist alike—is good conduct, and assume that we agree about its identity. Then of what relevance are these questions and definitions?

"Actually then only deliberate action, conduct into which reflective choice enters, is distinctively moral, for only then does there enter the question of better or worse," Dewey tells us.¹⁵ Such a statement can be recognized, again, as only a definition although it is offered as a pragmatic proposition beneath which philosophic undergarments borrowed from the Christians can be discerned. Dewey's conception flows from the idea that we will not call a man moral unless he has been "free" to sin. And, while one cannot quarrel with this definition any more than with those of Humpty Dumpty whose way with words remains so popular, some anomalous consequences should be noted. An Episcopalian rector, when asked whether, in his theology, a man could be said to be "moral" if he had never had the opportunity of evil, with little hesitation responded in the negative. This version of Christian ethic and its pragmatic translation should make life interesting since it must mean that the person who aspires to be "moral" should expose himself to temptation. A second anomaly of this definition of morals (and of the "dignity" attached to it) is that the actions of those who profess it are inconsistent with the definition itself. Individuals are called "moral" or "good" without assessment of the amount of evil they have resisted. And the traditionalist joins the determinist in taking every available precaution to "mould character," that is, to condition choice and thus to remove temptation.

But if acceptance of this definition produces anomalies for the theologian-philosopher, it also leads to barbed entanglements for the social scientist. A recent, excellent textbook maintains, with Dewey, that "unless men are free to choose between alternatives, they are not moral agents," and that a free choice is "reflective choice made in

the light of knowledge."¹⁶ The authors, to be sure, recognize that these are definitions, not propositions, but it is difficult to reconcile the *usefulness* of such definitions with what they have told us earlier in the volume namely: "The ethos not only sets the course of society; it infuses motivation into individuals as well. Its values are poured into us with . . . our mother's milk. Individuals are microcosms of culture."¹⁷ If this is so, if motives flow into man from his culture, if he bears in his preferences, emotions, and roots of action the imprint of its ethos, then how "reflective" can choice be, and how "available" the knowledge that would influence it?¹⁸

This dispute calls for study—clearly a possibility—that will weigh the extent to which the *moral quality* read into a man's life is based on his decisions, the extent to which the decisions are "reflective," and how often in such cases "knowledge" is available. Then, if we are to work with these definitions, we shall have to specify what we mean by knowledge being "available," and how much of what kinds of this available knowledge is generally required for a "free choice." When these questions have been investigated—and perhaps their mere phrasing is enough—it may be decided that the questions and the answers to them are unimportant. They probably cannot tell us anything we need to know if our concern is to produce more ethical behavior.

I believe that merely to have raised these questions is to indicate that neither the evaluation nor the control of human behavior requires that we concern ourselves with "moral agents" but only with men, and that most of the conduct about which we moralize is less a result of informed reflection than of wants, impulses, needs, and emotions, often imperfectly understood, conditioned in multiple ways, and operating in an environment over which we have the meagerest control. One can and does "make up his mind," but this is only a figure of speech, gratifying to

¹⁵ Ralph Ross and Ernest van den Haag, *The Fabric of Society*, New York: Harcourt, Brace, 1957, pp. 286, 297.

¹⁷ *Ibid.*, p. 99.

¹⁸ Although I may not fully agree with Leslie A. White's arguments on this point, they are at least consistent. See *The Science of Culture*, New York: Farrar, Straus, 1949.

¹⁵ John Dewey, *Human Nature and Conduct*, New York: Holt, 1922, p. 279.

those well-rutted beings who have never known the tragedy and stimulus of conflict. It would seem more useful to assume that "minds," too, are contingent, and usually "get made up."

Arguments that moral behavior is dependent upon "free choice" come more and more to appear as an expression of anxiety-prompted superstition¹⁹ or as part of an ideology—and ideology, we are told, functions to "justify" something.²⁰ In this case, cruelty.

3. *Is ethical behavior—including a sense of responsibility, duty, obligation—dependent for its production and maintenance upon the assumption of "free will"?*

Again, asking this question is almost to answer it, for we are aware that many societies have moralities, ideas of "right" and "wrong," without positing "free will." They simply hold forth ideals of conduct to which the socialized human responds with varying degrees of success. And, as with societies, so too with individuals whose careers have been strongly dictated by moral principle.²¹

The feelings presently attributed to "responsibility," the feelings of duty and oughtness and a strong sense of what one could never do, all these can be and are produced in man without reference to terms made meaningless by the assumptions of science and daily life. Man need not believe that he is "uncaused" to behave morally. He need not believe that he is "responsible for his character" in order to have developed a good one, nor does he necessarily become morally

apathetic because he abandons belief in this kind of "freedom."

These statements are hypotheses for which I believe there is evidence. But controlled observations are called for, first, to extend the type of study reported here in which the specific consequences of the beliefs of determinists and indeterminists are compared. If such comparison were made for both words and deeds, it is hypothesized that determinists would be found to be no less moral nor less energetic in their sense of duty than indeterminists.

Second, the ontogeny of morals can be studied in the ways, *mutatis mutandis*, that Lasker and Piaget have demonstrated.²² Study of this kind may show us how better to maximize good behavior and to minimize bad where, in greatest part, we are agreed on the meaning of each. We can then see whether the problem of moral development yields best to operations that assume free choice or to those that result from belief in causation, and whether or not the actor himself must assume that he is "uncaused" in order to act responsibly.

4. *Is "responsibility" relevant? To what?*

The term "responsibility" is used in diverse ways and its imputation lacks consistent grounds. Ascribing responsibility to one's self, for example, may involve different assumptions and reflect different emotions from its ascription to others, particularly when the "others" deviate from our codes.

In the latter case, it is a serious question whether or not, in planning how best to achieve public control of intolerable behavior—crime, delinquency, alcoholism, the chemical addictions—we need to determine the "moral responsibility" of the deviant. Such determination ordinarily represents a verdict as to the applicability of punishment for the breach of legal and moral commandments. But the grounds upon which jurists and moralists presently assign "responsibility" do not provide the most reasoned basis for recommending punishment. If "responsibility" were defined independently, and in such a way that its referent was regularly and

¹⁹ "The impression is often given that the more psychologists discover about our behaviour, the more we are found to be acting under compulsion; and thus we tend to imagine that the circle of our freedom is forever diminishing as psychology advances. Thus Hampshire writes: '... as our psychological and physiological knowledge of human actions and reactions increases, the range of human actions of which we can reasonably say "an alternative action was possible," or "he could have acted otherwise," necessarily diminishes.' If this impression is a true one—and it has yet to be shown to be false—then our anxiety is quite genuine." John Wilson "Freedom and Compulsion," *Mind*, 67 (January, 1958), p. 61 (Emphasis supplied).

²⁰ Karl Mannheim, *Ideology and Utopia*, New York: Harcourt, Brace, 1936.

²¹ L. F. Solt, "What Was Cromwell's Religion?" *The Listener*, 60 (September, 1958), pp. 335-336.

²² In B. Lasker, *Race Attitudes in Children*, New York: Holt, 1929; and Jean Piaget, *The Moral Judgment of the Child*, London: Kegan, Paul, Trench, Trubner, 1932.

intimately connected to the efficiency of punishment, there would be no quarrel with its determination. But if this is not the case, then the pragmatic question remains: Of what relevance is "responsibility"?

The publicly professed goals of punishment can be achieved without debating the "free" or "unfree" state of the actor and his "responsibility." Social protection, deterrence, and reform can probably be achieved more efficiently if we abandon these thoughtways, and, while "social solidarity" may yet require punishment, the relevance of "moral responsibility" to this end remains unclear.

"Responsibility" is a "good" word maintained on the grounds that it produces good effects. The truth may be that the concept is irrelevant for our purposes, that its continued invocation and attendant assumptions produce evil, and that, as with phlogis-

ton, the ether, and protoplasm,²³ we can obtain better results without it. The history of science is a history of the abandonment of such "necessary ideas."

It is suggested that the expected consequences of the assignment of "responsibility" in the control of illegal and immoral behavior be so phrased that we may test whether such attribution is essential to the goals we seek. Dispensing with "responsibility" under these circumstances will indicate its relevance, and it may be demonstrated, as this paper suggests, that the concept is a harmful honorific, good only for revenge. If, then, we know what we want, we may become "free" to ask whether revenge costs too much.

²³ Garrett Hardin, "Meaninglessness of the Word Protoplasm," *Scientific Monthly*, 82 (March, 1956), pp. 112-120.

RESEARCH REPORTS AND NOTES

DRINKING BEHAVIOR RELATED TO DEFINITIONS OF ALCOHOL: A REPORT OF RESEARCH IN PROGRESS *

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This is a report of certain findings arising out of pre-test studies aimed at developing an instrument for studying the self, significant others, and alcohol as objects in the individual's symbolic environment.¹ The major thesis of the larger study is that man behaves in a symbolic environment composed of objects which have been defined for him by significant others and in terms of "appropriate" behavior toward the object. It logically follows that a person's definitions of a given object in his symbolic environment embody his plans of action toward it and these, in turn, are indicative of his other behavior with respect to the object.

The aim of the larger study is to contribute to a better understanding of the many faceted phenomenon called alcoholism. In this effort, we, first, attempt to describe the prevailing drinking habits and related views on liquor and its use in the general population of Iowa. Second, we plan to treat the individual's quantity and frequency of drinking as a dependent variable and correlated with such factors as his conception of liquor and his conception of himself. The research being reported here deals specifically with the quantity and frequency of the person's drinking as it relates to his conceptions or definitions of alcohol.

At present the only known factor common to all alcoholics is the heavy consumption of alcohol. Logically, our understanding of alcoholism should be furthered by greater knowledge of the way in which alcoholic beverages are differentially viewed and used by the whole range of drinkers from the heaviest through the moderate users, and of the views of those who reject liquor in all forms. Such an approach

should help to understand the etiology of alcoholism and, perhaps, aid in the earlier detection of alcoholism or incipient alcoholics.

The limited analysis reported here of the subject's symbolic environment tests two hypotheses: that the individual's definitions of alcohol form a cumulative scale; and that his definitions of alcohol correlate with his report of the quantity and frequency of his drinking. Thus, the two major variables to be conceptualized and measured are the subject's definition of alcohol and his report of his use of alcohol.

The subject's definition of alcohol is conceptualized as the statements he makes about alcohol. A cumulative type scale measures this variable. The procedure for constructing the scale seems to be consistent with the logic of the symbolic interaction orientation, which dictates that the investigator inquire into the meanings that the *subject*, not the investigator, attaches to the objects in his symbolic environment.

Responses to an open-ended question are the source used to begin the construction of the cumulative scale. In an earlier study Mulford,² employing a symbolic interaction orientation, collected responses to the open-ended question: "What do alcoholic beverages mean to you? How do you define liquor? In answer to the question, 'What is liquor?' make some statements to complete the sentence, Liquor is. . . ." This form of question was suggested by the "Twenty Statements Test," which Kuhn uses to obtain definitions of self.³ Respondents could make as many as ten statements in response to the "Liquor is. . ." question. Analysis of the content of such responses by 146 college students brought into relief one category of statements, as expected. This category consists of those statements which indicate that alcohol is being defined as something that can be used as a means of rendering a situation, set of conditions or state of affairs more pleasant, desirable, or acceptable to the person. These state-

* This report is a slightly modified version of a paper presented at the joint session of the American Sociological Society and the Society for the Study of Social Problems in August, 1958. The research was made possible by the support of the Psychopathic Hospital and the Department of Psychiatry at the State University of Iowa.

¹ Other aspects of the study will be reported in subsequent papers.

² Harold A. Mulford, Jr., *Toward an Instrument to Identify and Measure the Self, Significant Others, and Alcohol in the Symbolic Environment: An Empirical Study*, Ph.D. Dissertation, State University of Iowa Library, 1955, microfilm.

³ Manfred H. Kuhn and Thomas McPartland, "An Empirical Investigation of Self-Attitudes," *American Sociological Review*, 19 (February, 1954), pp. 68-76.

TABLE 1. RELATIONSHIP BETWEEN DRINKING AND NUMBER OF STATEMENTS DEFINING ALCOHOL AS A MEANS OF ADJUSTMENT

	Non-drinkers	Drinkers	Totals
No adjustment statements	50	19	69
One or more adjustment statements	29	48	77
	79	67	146

 $\chi^2 : 16.47$

Q: -.63

Yates correction applied

P < .001

ments tend to distinguish drinkers from non-drinkers as shown in Table 1.⁴

However, since responses to open-ended questions are cumbersome to analyze, and since it is desirable to have a measure which distinguishes among persons with different consumption rates, we attempted to construct a cumulative type scale of the responses to the open-ended question.

There are suggestions in the literature that heavy consumption and alcoholism are associated with drinking to induce direct personal effects; that moderate and light consumption is associated with drinking for interpersonal or social effects; and that non-drinkers tend to define alcohol in terms of negative personal and social consequences.⁵ Further analysis of the responses to the "Liquor is . . ." question reveal that the heavy drinkers made more statements defining alcohol in terms of its direct personal effects ("It helps me forget"), moderate and light drinkers gave statements defining it in terms of interpersonal effects ("It helps me enjoy a party"), and non-drinkers define alcohol in terms of negative personal

and social consequences ("It is a social evil").

To test the hypothesis of their scalability, 22 of these statements defining alcohol were selected in approximately equal numbers from the three categories of statements, and a list was prepared similar to the one shown in Table 2. The respondent was instructed to indicate for each item whether or not he personally would make the statement. When responses from 49 college students enrolled in two introductory sociology courses were analyzed, a five-item Guttman scale was obtained which met all of the usual criteria, with the scale scores significantly associated with the subject's drinking behavior as measured by a quantity-frequency index, described below.

The nine best scale items were retained and combined with 13 new items to form a second list, which was administered to a new population of 85 college students. This procedure was repeated twice, making a total of four different lists of items which were administered to four different samples of students. In the four pre-test administrations responses were obtained from a total of 335 students. Each time the respondent's scale score was found to be associated with our measure of the quantity and frequency of his drinking.

The fourth and final list, consisting of 25 statements, was administered to 111 college students ranging from freshmen to seniors. Of the 25 items, 11 were found to scale in the conventional Guttman manner, meeting all of the Guttman criteria; the Coefficient of Reproducibility is .90. These 11 items include all but one of those listed in Table 2; the first item scaled but failed to meet the criterion of a minimum of 20 per cent positive responses. Seven of the 11 items (those marked with an asterisk in Table 2) may be traced back to the first pre-test administration, while the other four were acquired in subsequent administrations.

At this point, the conventional Guttman scaling procedure was abandoned in favor of the so-called H-technique, as developed by Stouffer.⁶ Although the Coefficient of Reproducibility on the 11-item conventional Guttman scale was acceptable, some 64 percent of the respondents made at least one error. This was partly a result of the fact that some items had similar frequencies and the rank positions of these items might therefore be expected

⁴ Mulford, *op. cit.*, p. 87.

⁵ See, e.g., Robert F. Bales, "Cultural Differences in Rates of Alcoholism," *Quarterly Journal of Studies on Alcoholism*, 6 (March, 1946), pp. 480-499; John W. Riley, Jr., Charles F. Marden, and Marcia Lifshitz, "The Motivational Pattern of Drinking," *Quarterly Journal of Studies on Alcoholism*, 9 (December, 1948), pp. 353-362; Giorgio Lolli *et al.*, "The Use of Wine and Other Alcoholic Beverages by a Group of Italians and Americans of Italian Extraction," *Quarterly Journal of Studies on Alcohol*, 13 (March, 1952), pp. 27-47; Charles R. Snyder, "Studies of Drinking in Jewish Culture," *Quarterly Journal of Studies on Alcohol*, 17 (March, 1956), pp. 124-143; Robert Straus and Selden Bacon, *Drinking in College*, New Haven: Yale University Press, 1953, Chapter 5.

⁶ Samuel A. Stouffer *et al.*, "A Technique for Improving Cumulative Scales," in Matilda White Riley *et al.*, *Sociological Studies in Scale Analysis*, New Brunswick: Rutgers University Press, 1954, Chapter 17.

to reverse themselves on subsequent administrations. Furthermore, the H-technique reduces the amount of spurious differences which might be found as we move from the college student pre-test population to the more heterogeneous population of the larger study. In brief, the H-technique increases the reliability of the scale. However, as Stouffer points out, it is not a technique by which one can lift himself up by his own boot straps, and we note again

a person with a scale score of II would also have responded favorably to contrived items III and IV, but not to contrived item I.

As to the meaning of the content of the scale items, in general the personal-effect statements are at the top of the scale—they are the least popular items. Moving down the scale, the meanings seem to shift from the direct personal effects of alcohol to those more closely associated with interpersonal or social situa-

TABLE 2. THE SCALE OF DEFINITIONS OF ALCOHOL

Contrived Item	Per Cent Agree		Method of Scoring
			Score+if;
I	15%	Liquor helps me forget I am not the kind of person I really want to be.	Agree on both
	22	Liquor helps me feel more satisfied with myself.	
II	26	*Liquor helps me get along better with other people.	Agree on any two
	28	Liquor makes me less concerned with what other people think of me.	
	33	*Liquor gives me more confidence in myself.	
III	55	*A drink sometimes helps me feel better.	Agree on any two
	57	*Liquor helps me enjoy a party.	
	58	Liquor gives me pleasure.	
IV	59	Liquor makes me less self-conscious.	Agree on any two
	62	*Liquor makes me more carefree.	
	75	*Liquor goes well with entertainment.	
V	78	*Liquor is customary on special occasions.	
		Failure to agree with any of the preceding items.	

$$\text{Coefficient of Reproducibility} = 1 - \frac{\text{errors}}{\text{Max. possible errors}} = 1 - \frac{5}{(111)(4)} = .989$$

$$\text{Coefficient of Scalability} = 1 - \frac{\text{Respondents making errors}}{\text{Respondents not making errors}} = 1 - \frac{5}{106} = .955$$

that the items do scale according to the conventional Guttman procedure.

Our H-type scale is shown in Table 2. Four contrived items were formed from the twelve single items. A respondent is scored positive on contrived item I if he responds positively to both of the single items which make up the contrived item. (This item is the most difficult to agree to, that is, it is the least popular item.) For contrived items II and III, a respondent is scored positive if he agrees with two of the three single items in each case; and for contrived item IV he is scored positive if he agrees with two of the four single items. The Scale Score assigned to each respondent corresponds to the number of the most difficult contrived item which he answered successfully. In most cases this means that the respondent also gave a favorable response to all contrived items less difficult to answer; for example,

"Liquor is entirely and completely a social evil" does not appear in Table 2, but it and similar items will scale at the extreme lower end of the scale if a negative response to such items is treated as a positive response. However, for the college student population it is a very extreme statement, with only four of 111 respondents giving positive responses. (All four of these respondents are non-drinkers.)

The preceding analysis of the content of the scale items appears to lend strength to the empirical findings, to be reported later, which support the hypothesis that people behave toward alcohol according to the way they define it.

The second major variable—the person's use of alcohol—was conceptualized as the subject's report of his usual frequency of drinking and the quantity of alcohol ordinarily consumed over a given period of time. To measure this,

TABLE 3. RELATIONSHIP BETWEEN SCALE SCORES AND Q-F INDEX SCORES

	Frequency:		Infrequent		Frequent		Very Frequent	Totals
	Quantity: Q-F Index Scores	None	Small	Medium or Large	Small	Medium or Large	Medium or Large	
Personal Effects	I	1	0	2	1	4	2	10
Personal and Interpersonal Effects	II	0	1	4	2	5	7	19
Interpersonal Effects	III	0	6	5	11	11	10	43
Interpersonal Effects	IV	3	10	4	1	2	2	22
None	V	7	9	0	0	1	0	17
Totals		11	26	15	15	23	21	111

$$\chi^2 = 35.7$$

$$Q = .87$$

$$P < .001$$

a quantity-frequency index (called the Q-F index) was employed, which is a slight variation of the index originally developed by Straus and Bacon in their study of college drinking.⁷ The index is based on the quantity of alcohol ordinarily consumed at a "sitting" and the frequency of such sittings during a given period of time. The following two questions were used to obtain the necessary data:

1. If you are not an abstainer, how often during the past year did you have one or more drinks?
 1 to 5 times 2 or 3 days a week
 6 to 12 times 4 or more days a week
 twice a month to once a week
2. How much of each type of alcohol beverage do you ordinarily consume at a sitting? That is, from the time you start drinking until you quit?
 Beer Wine Mixed drinks
 Straight Whiskey Straight Gin....
 Other (type and amount)

The Q-F index score ranges from 0 for the non-drinker to 5 for the heaviest drinker. To illustrate the scoring: one who reports that he consumes medium or large amounts—that is, at least three bottles of beer, three glasses of wine or three mixed drinks—two to four times per month would receive a Q-F index score of 4; or, if he reports consuming this same quantity (or more) more than once a week, he would receive an index score of 5.

It is well known that alcoholics, at least during certain stages in their drinking careers, will considerably understate the extent of their consumption. However, for purposes of this

study, the value of the Q-F index does not depend on the very heavy drinker's accuracy in reporting amounts consumed. If the subject admits to drinking three or more mixed drinks more than once a week, he fits the "heaviest-drinker" category.

While it is not assumed that a perfect correspondence exists between a respondent's Q-F index score and the actual quantity and frequency of his drinking, the index appears to be an adequate measure for ranking individuals for the purpose of testing the hypothesis that a person's definitions of alcohol are related to his reports of his drinking behavior. Interviews with respondents previously scored on the Q-F index indicate that those who report relatively heavy drinking actually imbibe more than those who report drinking to a lesser extent. Furthermore, checks of internal consistency reveal that certain groups reported in other studies to be relatively heavy drinkers show higher Q-F index scores.

Perhaps a more significant question is whether or not the measures of the two variables are logically independent. Inspection of the two Q-F questions given above and of the items making up the scale shown in Table 2 gives us no reason to suspect logical contamination.

We have demonstrated that a person's statements defining alcohol form a cumulative scale. Our other principal finding is the association between the scale scores and the Q-F index scores. The significance of this finding takes on added weight when it is considered that a similarly high order of association between these two variables obtained in each of the

⁷ Straus and Bacon, *op. cit.*, Chapter 8.

four pre-test administrations. In other words, the heavy drinkers consistently responded favorably to the direct-personal-effects items while the moderate drinkers, rejecting these statements, accepted the interpersonal-effects items. Non-drinkers rejected both of these types of items.

Further research designed to interpret this association might follow one of the following lines. First, it might be reasoned that the personal-effects drinker learns to use alcohol as a means of attaining a larger number of goals than the interpersonal-effects drinker. An alternative argument is that the personal-effects drinker has more opportunities to imbibe alone, that, in general, he does not limit his drinking to group situations in the manner of the social drinker.

In conclusion, this progress report supports the hypothesis of scalability of statements defining alcohol, and presents evidence strengthening the hypothesis that a person's reported quantity and frequency of drinking are associated with his definitions of alcohol. Thus, the general "symbolic interaction" assumption that an individual's definitions of an object embody his plans and execution of action toward the object is also strengthened.

THE PSYCHIATRIC ATTENDANT: DEVELOPMENT OF AN OCCUPATIONAL SELF-IMAGE IN A LOW-STATUS OCCUPATION

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Members of occupational groups often develop occupational self-images: sets of beliefs, attitudes, and evaluations regarding their work.¹ By stressing certain highly valued aspects of the work—the skill it requires, its social utility, the perquisites it affords—an occupational self-image can provide work motivation and work satisfaction.² The person in a high-status oc-

cupation is aided in maintaining a flattering self-image by the social prestige of his occupation. Occupations such as those of the physician and the business executive are widely known to require skill and to carry high income and pleasant working conditions. Low-ranking occupations, however, do not command favorable society-wide evaluations; the public evaluates many jobs as unappealing or, oftentimes, distasteful.

How then do people in low-status occupations maintain favorable self-images? A number of plausible answers may be suggested. One is that they do *not* maintain favorable self-images, but are dissatisfied with their work and would leave it if they could. Another is that they are dissatisfied with their work but project their aspirations onto their children.³ Still another is that they reject or fail to internalize the value of occupational success, perhaps seeking personal fulfillment in activities off the job.⁴ Probably all of these patterns are to be found; yet it also seems likely that many low-status workers are satisfied with their jobs and have favorable occupational self-images. In this paper we try to show one way in which work satisfaction among low-status occupational groups can come about.

Our hypothesis is that people in low-status occupations may seize upon some aspect of their work which is highly valued, either throughout the society or in the work subculture, and build a self-image around it. We present data showing the basis on which workers in a low-status occupation—psychiatric attendants—may maintain a favorable occupational self-image. Attendants tend to minimize the less glamorous features of their work and focus upon the most highly valued element in the hospital's subculture: care of the patient.

³ Both Chinoy and Guest find that automobile workers tend to be dissatisfied with their work. Reactions to this dissatisfaction include day-dreaming about starting businesses of their own, hoping that their children will find better work, and a sense of futility and resignation. See Ely Chinoy, "The Tradition of Opportunity and the Aspirations of Automobile Workers," *American Journal of Sociology*, 57 (March, 1952), pp. 453-459; and Robert H. Guest, "Work Careers and Aspirations of Automobile Workers," *American Sociological Review*, 19 (April, 1954), pp. 155-163.

⁴ See Allison Davis, "The Motivation of the Underprivileged Worker," in William Foote Whyte, editor, *Industry and Society*, New York: McGraw-Hill, 1946, pp. 84-106. See also Robert K. Merton's discussion of "ritualism" and "retreatism" in his *Social Theory and Social Structure*, Glencoe, Ill.: Free Press, 1949, pp. 125-149.

¹ For a survey and selected bibliography of studies of occupational self-images and ideologies, see Theodore M. Caplow, *The Sociology of Work*, Minneapolis: University of Minnesota Press, 1954, pp. 124-141.

² For a recent treatment of the ego-enhancing functions of occupational self-images, see Harvey L. Smith, "Contingencies of Professional Differentiation," *American Journal of Sociology*, 63 (January, 1958), pp. 410-414.

SOURCE OF DATA

The findings grew out of a study of psychiatric nursing practices in North Carolina hospitals. Attendants were interviewed about their reasons for choice of the job, their duties, and their attitudes toward work. Interviews were conducted with 81 men and 63 women, selected randomly to include 15 percent of the attendants in each of five hospitals: a state mental hospital for whites, a state mental hospital for Negroes, a Veterans' Administration mental hospital for both races, a private mental hospital for whites, and a psychiatric in-patient clinic for whites in a general hospital affiliated with a medical school.⁵ The findings presented below hold for all five hospitals: there are no differences between hospitals significant at the .05 level on a chi-square test.

CHOICE OF THE JOB AND WORK SATISFACTION

Each attendant was asked, in open-ended questions, why he had chosen the job, and why he had remained in the job of attendant. The answers to each question were classified as either intrinsic (pertaining to the work itself) or extrinsic (pertaining to the externals of the job).⁶ Answers which incorporated more than one reason were coded according to the first reason given.⁷ *Intrinsic reasons* included

such explanations as interest in understanding mental illness through contact with patients, humanitarian interest in patients' welfare, satisfaction of working with people rather than things, and affection or sympathy for patients. *Extrinsic reasons* included such explanations as better salary than for former job, not qualified for anything else, spouse or relatives worked in the hospital, convenient transportation from home to hospital, and friendship with co-workers.

As Table I indicates, the majority of attendants (82.6 per cent) gave extrinsic reasons for taking their jobs but less than half (46.5 per cent) of the entire group offered the same type of reason for remaining. Thus there is a marked over-all shift in the kind of reason emphasized: from extrinsic to intrinsic. More particularly, about half of those who took their jobs for extrinsic reasons changed their outlook and said that they remained for intrinsic reasons. Among the 17.4 per cent who took their jobs for intrinsic reasons, less than one-third (4.9 per cent) changed to extrinsic reasons for remaining. Despite the fact that this small group shifted in a contrary direction, a general trend toward the intrinsic rather than the extrinsic as reasons for remaining in the job seems to be clear. A chi-square test shows the over-all shift in pattern of the responses, from reasons for taking the job to reasons for staying in the job, to be statistically significant at the .001 level for men and women considered separately and for both sexes combined.⁸

The intrinsic reasons for remaining in their jobs, offered by 53.5 per cent of the attendants, focus on their personal relationships with patients. Except for one individual whose purpose was to gain intellectual understanding of mental illness, the reasons given by these attendants appear to reflect an occupational self-image of the kind hypothesized: one which places heavy emphasis on the importance of the attendant in patient care. Their reasons for taking the job are mainly extrinsic, suggesting that no such self-image existed before they began work. These findings suggest that many attendants acquired a favorable self-image, not from the society at large as people in high-status occupations often do, but from the subculture of the hospital.⁹

⁵ The North Carolina Psychiatric Nursing Survey was conducted in the summer of 1955 by the Social Research Section, Division of Health Affairs, University of North Carolina, under the direction of Harvey L. Smith, director, and Harry W. Martin, project coordinator. Martin drew the sample, arranged for interviews, and, with Alvin M. Katz, constructed the interview schedule; the authors of this paper interviewed the attendants. The study was sponsored by the American Nursing Association, the North Carolina State Nurses' Association, and the School of Nursing and the Institute for Research in Social Science of the University of North Carolina. The larger study, focused on nurses rather than attendants, is reported in Harry W. Martin and Ida Harper Simpson, *Patterns of Psychiatric Nursing*, New York: American Nurses' Foundation, Inc., 1956. The authors are indebted to Harvey L. Smith and Harry W. Martin for critical reading of the manuscript and to James M. Beshers, Morton B. King, Jr., Robert E. Clark, and Berton H. Kaplan for suggestions and advice.

⁶ The intrinsic-extrinsic dichotomy is an adaptation of the classification of goals presented in E. R. Hilgard and D. H. Russell, "Motivation in School Learning," 49th Yearbook, National Society for the Study of Education, 1950.

⁷ A justification for coding open-ended responses in this manner is reported by Zeisel, who states that tests have shown high correlations between item-mentioned-first tabulations and tabulations based

on all items mentioned. Hans Zeisel, *Say It With Figures*, New York: Harper, 1957, pp. 79-81.

⁸ The method used for assessing the significance of changes is taken from Quinn McNemar, *Psychological Statistics*, New York: Wiley, 1955, pp. 228-230.

⁹ Comparable situations, in which occupational groups maintain favorable self-images not shared

TABLE 1. REASONS FOR TAKING AND REMAINING IN THE JOB OF ATTENDANT

Reasons	Men (N=81)	Women (N=63)	All Attendants (N=144)
Took job for extrinsic reason, remained for extrinsic reason	46.9%	34.9%	41.6%
Took job for extrinsic reason, remained for intrinsic reason	38.3	44.5	41.0
Took job for intrinsic reason, remained for intrinsic reason	12.3	12.7	12.5
Took job for intrinsic reason, remained for extrinsic reason	2.5	7.9	4.9
Total	100.0	100.0	100.0

For men, $\chi^2=25.48$, d.f.=1, $p<.001$; for women, $\chi^2=16.03$, d.f.=1, $p<.001$; for all attendants, $\chi^2=40.96$, d.f.=1, $p<.001$.

The quotations below illustrate the kinds of reasons the attendants gave for taking and keeping their jobs.

Extrinsic Reasons for Taking the Job of Attendant:

Negro male attendant at the Negro state hospital: "I got to where I couldn't conduct the farm as I wanted to, because of my health and high blood pressure. Some of my friends had always worked here, and they pointed me here."

White male attendant at the white state hospital: "I wanted to quit farming; it's all work and no money and you're old before your time. My brother worked here and he told me about it. I had no notion what it was like when I started, but I liked it so I have stayed on."

Negro female attendant at the Negro state hospital: "The lady I worked for as a maid wanted me to find a better job. I didn't know of

another job I could do. If I had, I would have chosen it instead."

White male attendant at the veterans' hospital: "The pay looked all right and I wasn't trained for much else, so I thought I'd give it a try."

Intrinsic Reasons for Remaining in the Job of Attendant:

White female attendant at the private hospital: "Somebody has to help these people get well, and I feel it's our mission to do that."

White male attendant at the veterans' hospital: "... I ask myself how I would want my mother or my wife or my kids treated if they got mentally sick, and that's how I try to treat these people here. It could happen to anybody."

Negro male attendant at the Negro state hospital: "... They get to feeling so bad they'll just brood all day long unless somebody shows an interest in them. We try to talk to them and cheer them up when they are like that."

White female attendant at the white state hospital: "I have a real interest in the psychiatric nursing (sic) we do. The patients see us more than they see anyone else, and how we act with them is very important to their welfare."

ATTENDANTS' PERCEPTIONS OF THEIR DUTIES

A further indication of the attendants' self-image is the way in which they perceive their

by the general public but based rather on the values of work subcultures, are reported in Becker's discussion of the dance musician and Goodrich's discussion of the coal miner. "Art for art's sake" and sturdy independent skill are the values emphasized in the self-images described, respectively, by Becker and Goodrich. Howard S. Becker, "The Professional Dance Musician and His Audience," *American Journal of Sociology*, 57 (September, 1951), pp. 136-144. Carter Goodrich, *The Miner's Freedom*, Boston: Marshall Jones Company, 1925.

TABLE 2. ACTIVITIES MENTIONED AS MOST IMPORTANT AND MOST TIME-CONSUMING

Type of activity (In order of most patient-centered to least patient-centered)	Percentage of Attendants Mentioning Activity as Most Important			Percentage of Attendants Mentioning Activity as Most Time-Consuming		
	Men (N=79)	Women (N=62)	All Attendants (N=141)	Men (N=81)	Women (N=63)	All Attendants (N=144)
Interaction with patients	25.3%	32.3%	28.4%	6.2%	3.2%	4.9%
Physical care of patients	40.5	51.6	45.4	33.3	25.4	29.9
Supervision and observation of patients' behavior	24.1	11.3	18.4	12.4	14.3	13.2
Housekeeping and miscellaneous	10.1	4.8	7.8	48.1	57.1	52.0
Total	100.0	100.0	100.0	100.0	100.0	100.0

Chi-squares computed from contingency tables including data on most important and most time-consuming activities: for men, $\chi^2=32.64$, d.f.=3, $p<.001$; for women, $\chi^2=48.22$, d.f.=3, $p<.001$; for all attendants, $\chi^2=76.00$, d.f.=3, $p<.001$.

duties. They were asked, in open-ended questions, to name the *most important* and *most time-consuming* aspects of their work. The activities mentioned as most important differ somewhat from the activities cited as most time-consuming. Chi-square tests show the patterns of responses to the two questions to differ significantly at the .001 level, for men and women separately and for both sexes combined. The attendants' answers tend to stress activities directly connected with patient care as most important, although housekeeping tasks such as cleaning floors and making beds may be more time-consuming. Table 2 shows these findings. When the attendants were asked to name the most important duties, 73.8 percent of the 141 who responded mentioned care of patients or interaction with them as most important; only 7.8 per cent referred to housekeeping and miscellaneous tasks. But when they were asked to name the most time-consuming duties, 52 per cent of the 144 indicated housekeeping and miscellaneous tasks while only 34.8 per cent cited care of patients or interaction with them.

DISCUSSION

The data support the hypothesis that people in a low-status occupation can develop or maintain a favorable occupational self-image by focusing upon some highly valued aspect of the work situation. In the case of psychiatric attendants, emphasis is placed on care of the patient.

Professional training, such as that which doctors and nurses receive, not only provides technical competence but usually leads to an occupational self-image. The psychiatric attendant, however, has not undergone such extensive training. Therefore when attendants develop favorable occupational self-images, they are apt to be based on the workers' direct role in the primary function of the hospital, patient care.¹⁰ Cleaning floors and supplying linen closets have only an ancillary relation to patient care—understandably, most attendants do not regard these duties as their most important tasks although they are time-consuming.

This is not to imply that all attendants are entirely happy in their work. In our sample, 46.5 per cent of the attendants gave extrinsic reasons for staying in their jobs, and some of

those who gave intrinsic reasons may have felt ambivalent about their work. It seems, however, that a self-image based on patient care brings a measure of job-satisfaction to many attendants. Further research would be needed to determine the factors associated with intrinsic or extrinsic reasons for remaining on the job.

The self-image developed among psychiatric attendants serves the same functions as the ideologies of other occupations. It furnishes ego-enhancement and motivation. It appears likely that other low-status groups may develop self-images similarly, each grasping at whatever symbols of skill or social utility the situation affords.

ON STATUS INTEGRATION AND SUICIDE RATES IN TULSA

JACK P. GIBBS

University of California, Berkeley

AND

WALTER T. MARTIN

University of Oregon

The recent appearance of Powell's article on suicide and homicide in Tulsa, Oklahoma¹ poses a question of the applicability of the theory of status integration² to the Tulsa suicide data. Since the single article on the theory is extremely brief and presents few illustrations, and since some readers apparently have attempted to apply the major theorem, several tests are considered here.* This systematic exploration of the Tulsa data was undertaken with misgivings, since some of the "occupational categories" employed by Powell are not suited for an empirical evaluation of the theory.

The data presented in Table 1 of Powell's article make possible an examination of the relationship between simple measures of status integration and suicide rates for nine occupational categories at four points in time. Table 1 indicates the nature of one of the four tests.

It is possible to select evidence from the

¹ Elwin H. Powell, "Occupation, Status, and Suicide: Toward a Redefinition of Anomie," *American Sociological Review*, 23 (April, 1958), pp. 131-139.

² Jack P. Gibbs and Walter T. Martin, "A Theory of Status Integration and Its Relationship to Suicide," *American Sociological Review*, 23 (April, 1958), pp. 140-147.

* This report, as the authors have indicated in correspondence, thus is a reply to Berthold Brenner's communication, "Suicide and Occupation," *American Sociological Review*, 23 (October, 1958), p. 579. —The Editor.

¹⁰ On the possible importance of a group's relation to the primary function of an organization in establishing the prestige of the group within the organization, see Raymond W. Mack, "The Prestige System of an Air Base: Squadron Rankings and Morale," *American Sociological Review*, 19 (June, 1954), pp. 281-287.

TABLE 1. MEASURES OF STATUS INTEGRATION AND AVERAGE ANNUAL SUICIDE RATES PER 100,000 POPULATION BY OCCUPATIONAL CATEGORIES FOR WHITE MALES IN TULSA COUNTY, 1947-51*

Occupational Categories	M.S.I.** (Population)	Rank of M.S.I.	Suicide Rate	Rank of Suicide Rate
Professional-Manual	18,851	1	21.2	4
Sales-Clerical	12,111	3	9.9	6
Craftsmen (skilled labor)	14,537	2	8.3	7
Operatives (semi-skilled labor)	10,339	4	23.2	3
Unskilled labor	3,020	7	53.0	2
Service	2,431	8	8.2	8
Retired	2,215	9	63.0	1
Agriculture	3,637	6	11.0	5
Student	9,562	5	6.3	9

* Data from Powell, *op. cit.*, Table 1.

** Measure of status integration. For comparisons within a column the M.S.I. for any cell is simply the number of cases in the cell. Thus, for this kind of comparison only, the numbers need not be reduced to proportions of the column total.

Tulsa data that is apparently contrary to the theory. The category "Professional-Manual," for example, has the highest integration measure of all the occupational categories (see Table 1) and consequently, according to the theory, should have the lowest suicide rate. Actually, the rate of this category is surpassed by only three of the nine categories. Other negative cases can be detected. It would be most unfortunate, however, to restrict attention to incorrect predictions without also giving due consideration to evidence which does support the theory.

When the nine categories are considered together, the ρ between the ranks shown in Table 1 is $-.25$. In similar tests, using data from the other panels of Powell's Table 1, ρ proves to be $-.33$ for 1937-41, $-.27$ for 1942-46, and $-.52$ for 1952-56. These correlations are low; however, each conforms to the inverse relationship anticipated by the major theorem.

The writers' doubts regarding the value of the above tests stem principally from the nature of Powell's occupational categories. Some of the categories ("retired" and "student") are not occupations but labor force designations which require separate treatment. Two categories, service and agriculture, are closer to industrial classes than to occupations. Finally, the professional-manual category lumps together two distinct status categories, producing an artificial measure of status integration.

In recognition of the deficiencies of the data additional tests were conducted in which four of the unsuitable categories were excluded and the professional and managerial statuses treated separately.³ Since Powell did not give suicide

rates for the professionals and managers separately, their ranks in Table 2 have been determined on the basis of their reported combined rate and Powell's observation that the professions have a lower rate than the managers. The managerial category includes proprietors and officials as well as managers proper.

For the years 1937-41 the ρ between the ranks of the measures of status integration and the suicide rates by occupational categories, as shown in Table 2, is $-.77$. An identical value of ρ holds between the ranks shown for 1947-51 in the right hand side of the table. Analysis of the data in Powell's table for 1942-46 and 1952-56 shows that for both periods the value of ρ is also $-.77$. Thus, although the ranks are not constant for all of the periods, a consistently high relationship holds.

As a further test of the major theorem, we consider the relationship between the ranks of status integration and suicide rates by specific occupations among the white-collar and blue-collar classes in Tulsa.⁴ While rarely available, suicide rates for specific occupations are extremely important since use of the usual broad categories may conceal differences among distinct statuses.

The relationship between the ranks in Table 3 provides clear-cut support for the theory of status integration; here ρ is $-.94$ among the white-collar occupations and $-.83$ among the blue-collar occupations. When these two categories are considered together, ρ is $-.90$. The results of these tests compared with the earlier results demonstrate the need to employ specific occupational statuses.

Suicide rates are also presented by age groups in Tulsa County. Here, in contrast to Powell's treatment, the theory of status integration gen-

³ The lumping together of sales and clerical workers, because of their close resemblance as occupational categories, seems to be justifiable.

⁴ The data are drawn from Powell, *op. cit.*, p. 136.

TABLE 2. MEASURES OF STATUS INTEGRATION AND AVERAGE ANNUAL SUICIDE RATES PER 100,000 POPULATION BY SELECTED OCCUPATIONAL CATEGORIES FOR WHITE MALES IN TULSA COUNTY AT TWO POINTS IN TIME *

Occupational Categories	M.S.I. 1940 (Population)	Rank of M.S.I.	Suicide Rate 1937-41	Rank of Suicide Rate	M.S.I. 1950 (Population)	Rank of M.S.I.	Suicide Rate 1947-51	Rank of Suicide Rate
Professional	4,385 ^a	5	<63.0	2	8,853 ^b	5	<21.2	4
Managerial	7,077 ^a	4	>63.0	1	9,998 ^b	4	>21.2	3
Sales-Clerical	10,988	1	16.4	6	12,111	2	9.9	5
Craftsmen	7,975	2	17.7	5	14,537	1	8.3	6
Operatives	7,645	3	26.1	4	10,339	3	23.2	2
Unskilled	2,535	6	39.4	3	3,020	6	53.0	1

* Unless designated otherwise the figures shown have been drawn from Powell, *op. cit.*, Table 1.^a From *Sixteenth Census of the United States: 1940, Population*, Vol. II, Part 5, pp. 854-856.^b From *U. S. Census of Population: 1950*, Vol. II, Part 36, p. 206.

TABLE 3. MEASURES OF STATUS INTEGRATION AND AVERAGE ANNUAL SUICIDE RATES PER 100,000 POPULATION FOR SPECIFIC OCCUPATIONS IN TULSA, 1937-56 *

Occupations	White-Collar				Blue-Collar			
	M.S.I. (Pop.)	Rank of M.S.I.	Suicide Rate	Rank of Rate	M.S.I. (Pop.)	Rank of M.S.I.	Suicide Rate	Rank of Rate
Pharmacists	162	6	120	1	402	6	86.9	1
Physicians	309	5	83	2	974	5	25	2
Nurses	776	3	38	3	1,154	4	17	3
Lawyers	544	4	36	4	2,847	2	12	4
Engineers	1,953	2	15	5	3,776	1	10	5
Accountants	2,054	1	7	6	1,804	3	5	6

* Data from Powell, *op. cit.*, Table 3.

TABLE 4. AVERAGE ANNUAL SUICIDE RATES PER 100,000 MALES, 1937-56,* AND MEASURES OF OCCUPATIONAL INTEGRATION BY AGE GROUPS, 1950,** IN TULSA COUNTY

Occupational Categories	Age Groups					
	14-24	25-34	35-44	45-54	55-64	65 and over
Professional, Technical	.0785	.1625	.1465	.1251	.1149	.1154
Farmers, Farm managers	.0051	.0109	.0174	.0215	.0347	.0793
Managers, Officials, Proprietors	.0365	.0975	.1726	.2151	.2283	.2219
Clerical	.1309	.1073	.0707	.0768	.0729	.0572
Sales workers	.1247	.0923	.0839	.0770	.0798	.0994
Craftsmen, Foremen	.1744	.2445	.2386	.2199	.2056	.1755
Operatives	.2441	.1822	.1635	.1362	.1064	.0611
Private household workers	.0026	.0014	.0018	.0025	.0038	.0029
Service workers	.0760	.0407	.0439	.0621	.0855	.1201
Farm laborers, Foremen	.0236	.0093	.0078	.0079	.0091	.0114
Laborers	.1036	.0515	.0534	.0559	.0590	.0558
Proportions summed (ΣX)	1.0000	1.0001	1.0001	1.0000	1.0000	1.0000
Integration measure (ΣX^2)	.1473	.1534	.1521	.1482	.1427	.1342
Rank of ΣX^2	4	1	2	3	5	6
Rank of suicide rate	6	5	4	2	3	1
Suicide rate	6*	10	20	34	29	40

* The suicide rates shown here have been estimated from Powell, *op. cit.*, Figure 1.

** Source of occupational data: *U. S. Census of Population: 1950*, Vol. II, Part 36, pp. 200-201. The figures shown relate to Tulsa S.M.A., which comprises Tulsa County.

* Suicide rate is for the age group 15-24.

erates a highly specific proposition: suicide rates by age groups vary inversely with measures of the degree of occupational integration within the age groups. Table 4 provides the necessary data for a test of this proposition with respect to the male suicide rate.⁵

The suicide rates of Tulsa males by age show a pattern characteristic of the United States as a whole. With only one exception, there is a uniform increase in suicide with increasing age; and, significantly, with only one exception, there is a uniform decrease in measures of occupational integration by age groups. The ρ between the ranks of the two (as shown in Table 4) is $-.54$.

The one exception to decreasing occupational integration poses a problem not covered in the brief report on the theory. The consequences of lack of status integration doubtless are not instantaneous; a superior test of the major theorem would take into account the temporal dimensions of status occupancy. The need for this type of control is suggested by the Tulsa

data. The outstanding exception to the relationship depicted in Table 4 is the age group 14-24. The measure of status integration (ΣX^2) is low for this age group, but the ages of the persons indicate that they have been exposed to this condition briefly; consequently, it is not altogether surprising to find that this age group is the exception to the correlation between low status integration and high suicide rates. Further, there is the possibility that this age group has a high degree of marital and parental integration, which compensates for their low degree of occupational integration.

When the age group 14-24 is excluded from Table 4, the ρ between the ranks of the measures of occupational integration and suicide rates is $-.90$.

Concerning the question of evidence, it should be noted that none of the occupants of six specific occupational statuses in Tulsa, all with low measures of integration, committed suicide.⁶ However, in this instance the number of cases is small, and, moreover, several important dimensions of status integration (parental, religious, marital) are not taken into account. Although substantiating data are not available, there is reason to believe that five of the six occupations have very high suicide rates in the United States as a whole. The remaining status,

⁵ It should be noted that occupational categories make up the rows of Table 4. Since movement in and out of statuses is basic to the theory, only achieved statuses should appear in the rows of a status integration table. Ascribed statuses are always entered at the head of table columns, where they may appear in combinations with achieved statuses.

⁶ Powell, *op. cit.*, p. 135.

clergyman, would not be expected to show many suicides because of a high degree of marital integration.

As a final observation, the writers are at a loss to see how or in what way Powell's observations constitute an explanation of the magnitude of suicide rates. While his paper is a contribution to the study of the phenomenon, there are some grave questions as to how the explanations offered can be objectively evaluated. For one thing, although Powell invokes

numerous concepts, there is no measure of an independent variable in the study. There are only differences in suicide rates and their attribution to one cause or another. However intellectually satisfying the explanation that anomie (which is said to result from both dissociation and involvement) results in a high suicide rate, the validity of the claim remains in question until some measure of the independent variable is devised so as to make possible the tests required for concrete demonstration.

COMMUNICATIONS

A NOTE ON TOENNIES' *GEMEINSCHAFT UND GESELLSCHAFT*

To the Editor:

Readers of the latest edition of *Gemeinschaft und Gesellschaft* (1935) may have wondered about the frequent apparent digressions of the author from the topics indicated in the subtitles of sections. For example, in Book Two, Section 31 is subtitled *Kuenstlerischer Geist* ("Artistic Spirit"). Although the opening statement equates "natural will" with artistic spirit, the Section itself is devoted to an elaboration of many points, including a discussion of the work of the teacher and adviser. Similarly, in Section 41, *Kinderarbeit* ("Child Labor"), there is a cogent paragraph about the future capitalistic *Gesellschaft* which bears no relationship to the lines about child labor. Again, in the Conclusions and Outlook, Section 2 is devoted to *Aufloesung* ("Dissolution"). From the subtitle itself, one cannot be certain just what is "dissolving" (if anything). The Section contains, among other passages, a discussion of folk culture and the *Gesellschaft*-like civilization.

It should be noted that these subtitles appear only in the 1935 German edition of *Gemeinschaft und Gesellschaft*. In the German editions of 1926 and 1920 the Table of Contents contains detailed descriptions of the contents of each "Chapter" or "Section." The text, however, contains no subtitles; sections are simply numbered. It is apparent that in the 1935 edition certain descriptive terms were selected more or less at random from the table of contents of the 1920 edition and utilized as subtitles. How this happened remains a mystery to be solved by a future biographer of Ferdinand Toennies.

Charles P. Loomis based his translation on the edition of 1935, taking of course the latest edition to be the most authentic one. The excellent quality of Loomis' translation of this very difficult work is attested by the fact that a British edition and a second American edition have been published in recent years.

It is unfortunate that neither the translator nor the senior author of this note, who read the translation in manuscript, nor apparently any reader, noticed the discrepancies between subtitles and content of sections, until a question raised by the junior author of this note led to their discovery.

For the benefit of future students of Toennies' work, both in Germany and in the English speaking world, we thought it appropriate to publish this communication.

RUDOLPH HEBERLE AND A. F. BORENSTEIN
Louisiana State University

PARTICIPANT OBSERVATION AND THE MILITARY: AN EXCHANGE *

A QUESTION OF PROFESSIONAL ETHICS?

To the Editor:

In their article on "Participant Observation as Employed in the Study of a Military Training Program," the authors report on a project conducted under the auspices of the Air Force. They write: "Certain officers wished to gain a better notion of how basic technical training were lived, understood, and felt by new airmen. Hence . . . a plan was drawn and approved for the utilization of a participant observer. . . . To accomplish this purpose it was decided that a research officer should 'enlist' as a basic trainee. He would be a fullfledged member of the group under study, his identity, mission, and role as a researcher unknown to everyone. . . ." (pp. 660-661) The authors add that "in deliberately cultivating a second self the research observer was engaged in something superficially like intelligence work or espionage. But there was a very important difference in goal for, in this case, it was a general understanding of a significant subculture. . . . It was simply to gather a body of previously unavailable information and to interpret it in a way that might be helpful both to the military and to social scientists." (p. 664)

In other words, the military decision makers wished to gain some information about their subordinates not otherwise accessible to them. To this end they planted a social scientist among their unsuspecting subordinates. The participant

* This exchange refers to Mortimer A. Sullivan, Jr., Stuart A. Queen, and Ralph C. Partick, Jr., "Participant Observation as Employed in the Study of a Military Training Program," *American Sociological Review*, 23 (December, 1958), pp. 660-667. The titles of the communications are my own—the authors are in no way responsible for them.—*The Editor.*

observer hence became in effect an informant, in the service of the military. Shades of the Garrison State!

May I submit that I consider this report to exhibit disregard for professional ethics. The same issue of the *Review* contains a Report of the Committee on Training and Professional Standards of the American Sociological Society in which is noted that "it has nothing to report at present concerning other matters, such as professional ethics. . . ." (p. 703) May I suggest that this Committee might find it profitable to consider the above article as a point of departure for the consideration of professional ethics?

LEWIS A. COSER

Brandeis University

DANGEROUS AND DIFFICULT ENTERPRISE?

To the Editor:

The article on participant observation in a military program is remarkable to me in a number of respects.

For one thing, the authors make it sound as if the undercover type of participant observation is extremely rare. Yet, without any careful search, I can think of a number of studies of this type in recent years. There is Mann's article on the marine radioman, Caudill's study of the psychiatric patient's role, the book *Why Prophecy Fails* by Festinger, Riecken, and Schachter. A number of dissertations on occupational groups at the University of Chicago are based entirely or largely on information collected by students who worked at the job they studied without revealing their professional interest to their fellow workers. . . . Some of these people took their jobs mainly to make money to pay tuition and living expenses, and then decided to use the opportunity to collect data for a study of an occupation. But the priority of their other motives does not detract from the value of their observations. In my own current study of the social psychology of the treatment of tuberculosis, I have collected my most valuable data while a tuberculosis hospital patient and later as a tuberculosis hospital attendant, in each case keeping daily notes on my observations and experience without the knowledge of the persons [being observed].

In the second place, Sullivan, Queen, and Patrick make such secret observation sound extraordinarily difficult. Note the elaborate nine-month preparation of their observer. To cite again my own experience, I made no preparation and took no training for the observational roles I played. For example, my vocabulary and English usage were much better

than those of my fellow patients and, later on, of my fellow attendants and even of the nurses who supervised us. Yet I made no attempt to disguise my speech or modify my vocabulary and I had no difficulty mixing comfortably with my temporary colleagues. The several University of Chicago sociologists I know who have engaged in such "undercover research" never mentioned making any special preparation for their participant role and, with one exception, had no difficulty carrying out their study.

I am quite sure that the observer airman could readily have stepped into his role despite some deviations from the "average" without any noticeable effect on his observations. I believe these researchers have entirely too narrow a conception of people's tolerance of somewhat deviant behavior.

It is my opinion that for studying the dynamics of complex social interaction there is no substitute for participation in the activities of the group (or groups) in which one is interested. I am afraid that the . . . article is more likely to scare off social scientists who have had no experience with this approach than it is to win more recruits. I want to say to such people: It's not nearly as difficult as they make it sound. Give it a try yourself.

JULIUS A. ROTH

The University of Chicago

ETHICS—AND DIFFICULTIES: REPLIES TO COSER AND ROTH

To the Editor:

Mr. Coser is evidently disturbed because he believes that an Air Force research organization was used by an Air Force training activity for the purpose of spying. The participant observer approach to the training activity's sincere interest in trainees' attitudes towards indoctrination and technical schooling was the recommendation of a member of the *research organization*. Had the officials of the training activity wanted an informant, it would have been simple enough for them to have "planted" one of their own men with instructions to "get names."

We respect and share Mr. Roth's enthusiasm for participant observation and we would be as distressed as he, were our article to deter anyone from contemplating this approach. We certainly concur with any statement that, *without the broadest meaning of the term*, participant observation is neither rare nor difficult. There are, however, instances in which the

use of this research method necessitates involving certain complexities as to placing the observer in the field and providing for data reporting and analysis. . . . Some of the special considerations confronted in planning and carrying out our study include the following:

1. *Inaccessibility of the institution to be observed.* For obvious reasons a civilian sociologist, no matter what his qualifications, could not have been "enlisted" into the Air Force for the purpose of conducting the study. A knowledge of the existing structure, intended training goals, and current problems of the service was essential.

2. *Necessity of engaging in one hundred per cent participation.* All uses of participant observation do not require the researcher to become as immersed in the activities of the environment as in our case. Adoption of a total participating role was necessary and desirable here [because] without it, the observer would not have been accepted by his "superiors" or "fellow trainees" as a true participant in the program, his capacity as a researcher might have been thus detected (and probably misunderstood), and his role reduced to occasionally recording unconnected observations. Consequently, those data regarding the earlier structuring process and the relative adjustment of trainees would have escaped timely notice.

3. *The required "elaborate" preparation of the observer.* . . . in the case of this study, preparation was extremely important. The observer's knowledge and experience in matters pertaining to his training and service as an Air Force officer and his educational background, while essential to contributing to data analysis, would have certainly led to his early detection. . . . Inadvertently revealing cognizance of military customs . . . or procedures might have raised suspicion or resulted in the assignment of the observer to a leadership position—an occurrence which had to be carefully guarded against. It was the recommendation of William Foote Whyte that the observer strive to achieve rapport among both official and unofficial trainee leaders, but not become a leader himself. Had it been decided to "enlist" the observer without such preparation (assuming anonymity could be otherwise assured), he would have most certainly been placed in an organization with his . . . educational peer. . . . Within the observer's organization there were, to be sure, some "older" trainees and some men with some college education. As the team of trainees took shape, these men ultimately were absorbed into both official and unofficial activities common to other trainees. During the first weeks of observing

each group, however, the adolescent trainees kept very much to themselves, talked about their common interests and their reactions to the new environment. It was not enough for the observer to be in the same program with them. He had to be to some extent "one of the boys."

This study was no doubt unique with respect to the necessary protection against deviant behavior on the part of the observer. If military security and traditionalism have anything in common with other-directed adolescence, [it] is a characteristic lack of tolerance of (even somewhat) deviant behavior.

4. *The decision to utilize collaterally a questionnaire.* As reported in the article (p. 666), at four times during the field study, a questionnaire was administered to the trainees in the observer's organization. The questionnaire, which was primarily prepared, administered, and analyzed by the Air Force's civilian sociologist, was an integral part of the study.

5. *Use of the field study "research team."* The remaining members of what we have called the "research team" and the Air Force's civilian sociologist worked diligently with the observer to prepare him for the job of reporting. The analysis schedule described . . . (p. 665) enabled the "team" to consider its data and realign its thinking as training progressed. Thus the use of the "team," while important to the study, may have made the task appear more "difficult."

Participant observation, then, is only as "difficult" as conditions require. It is only "rare" in its more extensive application.

MORTIMER A. SULLIVAN, JR.

University of Buffalo

NO "GARRISON STATE"—DIFFICULTIES, YES:
REPLIES TO COSER AND ROTH

To the Editor:

It is apparent that Mr. Coser is unduly apprehensive and has leaped to quite unwarranted conclusions. No garrison state is in the making! In fact, the partners in the participant-observer study of the training program for enlisted men in the Air Force include members in such anti-garrison-state organizations as the American Civil Liberties Union, Americans for Democratic Action, and the American Association of University Professors. The purpose of the study was not to "get anything on anybody," but to contribute to understanding and improvement of the training program. No individual airman and no individual military unit has been identified in any of our reporting. It was clearly understood both by officers of the Air Force, with whom the study was planned, and by those of us who took part in the project, that our findings

would be completely anonymous and impersonal. Our report is couched in terms of the subculture and informal organization of the enlisted personnel, and the social processes that go on during the periods of basic and technical training. We are sure that it has resulted in no disciplinary action against any individual or any group. We hope that it has contributed to some modifications of the training program which will make it both more useful to the Air Force and more satisfying to the enlisted men. As social scientists we have gained new insights into the relation of formal and informal organization, and the functions of patterned evasions. We think that we have a more adequate conception of the possibilities and limitations of participant observation as a research procedure. Some of these things will be set forth in a second article. . . . We hardly see how there can be anything "unethical" about this.

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It is interesting that the two critics of our article adopt almost opposite positions. Mr. Coser says in effect, we have introduced something novel, deviant, and even unethical. Mr. Roth, on the contrary, holds that we have done nothing unusual except to make harder work of the project than was necessary: procedures like ours are commonplace and to be taken for granted.

Mr. Roth makes two points which seem to me useful supplements or corrections to our article: there have indeed been other studies by participant observers; in some cases they have not been extraordinarily difficult.

As to the difficulties of our particular project, I feel freer to speak than my associate who actually was the participant observer. When "in the field" he must have worked more than 100 hours a week. He did all the military, "house-keeping," and educational tasks assigned to airmen in training; in addition, he sat down nightly to review the day's experiences and to dictate his notes. He did have something of a problem to transform himself from a 27-year-old, college trained, commissioned officer into a 19-year-old, near-delinquent, high school graduate. Also it was not easy to make his absences for dictation and conferring inconspicuous. The development of a common universe of discourse and of a mutually acceptable *modus operandi* presented numerous problems, perhaps peculiar to our team. While we may have overemphasized the difficulties, and while some other cases of participant observation we know about have been easier, it is our judgment that this technique is not to be taken lightly.

Withal, we are grateful to Mr. Roth for his pointed comments.

STUART A. QUEEN

University of Wichita

THE PROFESSION: REPORTS AND OPINION

OFFICIAL REPORTS AND PROCEEDINGS

Minutes of the Meeting of the Executive Committee

February 23, 1959—New York City

Present at the Meeting were: Kingsley Davis, Chairman, Howard Becker, John Clausen, W. Fred Cottrell, Wilbert Moore, Charles Page, John Riley, Irwin Sanders, Robin Williams, Donald Young, Matilda Riley, *ex officio*. Also in attendance were: Wellman Warner, Chairman, Classification Committee; Talcott Parsons and Guy E. Swanson, Committee on the Profession; Edgar Borgatta, Leonard Cottrell, Section on Social Psychology.

1. The current draft of the Recruitment Brochure was accepted with thanks to Raymond Bowers and his Editorial Board. This draft will be held by the Executive Committee pending the analysis of the Society's membership to be made in conjunction with the 1959 Directory and the development of the work on sociology as a profession by the Committee on the Profession.

2. The Committee discussed the present procedures for electing the Society's delegates to the Council of the International Sociological Association. It was generally agreed that no action was required at the present time.

3. The Executive Office was authorized to distribute the 1959 Directory of Members without charge to Associate Members of the Society as well as to Active Members and Fellows.

4. In response to a report from Leo Kuper that the South African government is forbidding the importation of a number of books including some of sociological interest, it was suggested to the Editor of the *Review* that he might invite Professor Kuper to write a note for publication on this subject. The general problem of the banning of books was referred to the Committee on the Profession.

5. In regard to relations with affiliated societies, the President was encouraged to hold again this year a luncheon meeting of officers, and the Executive Office was instructed to furnish as many services as possible (though not always without charge) and to promote national Society membership among the members of the affiliated societies. No action was taken on the

question of possible joint membership arrangements.

6. The Committee discussed a procedure for determining "major commitment to sociology." In cases of members whose Active status rested originally upon academic achievement or experience in related fields (not sociology) and whose advancement to Fellowship comes into question in 1960 and following years, it was decided to recommend to the Council:

- (a) That the Executive Office should work with the Chairman of the Classification Committee and the Secretary to study such cases (about fifty to one hundred each year) and to formulate proposed criteria of "major commitment;"
- (b) That these proposals be submitted to the Classification Committee for review and to the Council for general approval; and
- (c) That any member classified as eligible for Fellowship under this procedure shall have the option of either becoming a Fellow (if he considers himself to have a major commitment to the field of sociology) or remaining an Active Member.

7. The Committee reviewed a memorandum from the Classification Committee on "Redefinition of Membership Classification." It was the sense of the meeting that:

- (a) In regard to the criteria for *Active Membership* the memorandum provides the rationale and specifies the criteria approximately as these are now being applied under the action taken by the Executive Committee at its meeting on November 4, 1958. The Office was instructed to try out these criteria as set down in the memorandum (with the exception of the limitation of "related fields" to social psychology and social anthropology), to refer difficult cases to the Chairman of the Classification Committee and the Secretary as a Court of Appeals, and to refer to the Executive Committee any cases which cannot be handled under the existing rules.
- (b) In regard to the proposed subdivision of the category of *Associate Members* into "Members," who are junior sociologists and who vote, and "Associates," in order to broaden the membership base of the Society, the Classification Committee was asked to study the matter further and to report back to the Council at a later date.

8. On the matter of reduced dues for foreign members as referred to the Executive Committee by the Council, the Executive Committee voted

to propose a By-Law change as outlined by the Classification Committee so as to add the following section to Article I of the By-Laws.

"Dues of members from other countries who are residing abroad may be reduced at the discretion of the Council."

9. In line with the previous actions of the Executive Committee and Council, the following statement of policy was approved as presented by Guy Swanson, Chairman of the Sub-committee on Implications of Legislation Certifying Psychologists:

The American Sociological Society:

- (a) Shares the American Psychological Association's desire to protect the public against charlatans and incompetents;
- (b) Questions whether legal certification by whole title provides the intended protection for the public; and
- (c) Does not, in principle, approve measures which place legal restrictions on the use of titles referring to traditional areas of intellectual and scholarly pursuit.

In view of the adoption by the American Psychological Association of a policy favoring legal certification by whole title, however, the American Sociological Society will:

- 1. Seek exemption from such certification for sociologically-trained social psychologists who are properly qualified and who wish to engage in private practice;
- 2. Set up a program of certification by the Society to certify those properly qualified social psychologists who wish to offer their services as practicing social psychologists for a fee; to formulate a statement of minimum qualifications for such practice; to develop a code of ethics; and to handle violations under this code.
- 3. Provide expert witnesses who will, as court cases involving sociologically-trained social psychologists may arise, inform courts concerning appropriate ethical standards for persons in private practice.

The Executive Committee also approved the following draft exemption clause as exemplifying the type of legal provision it urges be included in all state laws which certify psychologists by whole title:

Nothing in this act is to be construed as restricting the use of the term "social psychologist" by any person (1) who has been duly graduated with a doctoral degree in sociology or social psychology from an institution whose credits in sociology or social psychology are acceptable by a state-supported college or university in (state), and (2) who has passed comprehensive examinations in the field of social psychology as a part of the requirements for the doctoral degree or has had equivalent specialized training in social psychology, and (3) who has notified the board of his intention to use the term "social psychologist" and filed with the board a statement of the facts demonstrating his compliance with prerequisites (1) and (2) of this subsection, and

(4) who is a citizen of the United States or has declared his intention of becoming one, and is at least 21 years of age.

The exception of this subsection shall not be available to any person who has been found by a court of this or any state of the United States (i) to have committed acts of gross immorality, (ii) to be habitually intoxicated, (iii) to be an habitual user of narcotics or other habit-forming drugs, (iv) to be insane, (v) to have been convicted of a felony or other crime involving moral turpitude, or (vi) to have engaged in dishonorable or unprofessional conduct. An action to determine whether any person asserting the exemption of this subsection has committed one or more of the acts listed in (i) through (vi) may be brought by (the prosecuting attorney of any county in this state).

The Executive Committee voted by acclamation its appreciation for the statesmanlike quality of the work of this Sub-committee.

10. The Committee voted thanks on behalf of the Society to the Legislative Research Center of the University of Michigan for its assistance in dealing with the legal certification of psychologists, and instructed the President to write a letter communicating this resolution.

11. Leonard Cottrell and Edgar Borgatta reported for the Section on Social Psychology on their work in drafting a code of ethics and a procedure for certification of social psychologists offering their services at a fee. This draft will be ready for consideration by the Executive Committee in the near future.

12. Talcott Parsons reported on the other work now being undertaken by the various Sub-committees of the Committee on the Profession.

13. Following the Council's mandate to give further specific consideration to possible rules and procedures for sections, both present and impending, the Executive Committee took the following actions:

- (a) The Executive Office was instructed to facilitate the organization of new sections in appropriate cases by such devices as sending notices to the *Review*, enclosing a petition in a regular membership mailing, or providing meeting space at the annual meeting. Such cooperation should not be provided on any substantial scale, however, until the Executive Committee or the Council has had an opportunity to consider the particular proposed section with reference to the overall structure of the Society.
- (b) Persons wishing to be members of sections must also be members of the Society, although they need not necessarily be Fellows or Active Members unless a given section so rules.
- (c) The existing By-Law procedures for forming sections were reviewed and it was agreed that applications from organizations or affiliated societies, as such, would be inappropriate.
- (d) A special day is to be set aside at the annual meetings for organizational meetings of sec-

tions, so that these will not interfere with the regular sessions of the program.

- (e) The Society's Program Committee will cooperate with sections in planning the presentation of fields of interest at the annual meetings of the Society. A section's program will, however, be under the general control of the Program Committee and must be cleared with it.
- (f) The Society's rules restricting multiple participation in the annual meeting program should be extended to cover any programs set up by sections.
- (g) No section should state a formal rule barring from its annual meeting program papers submitted by non-members of the section.

14. An invitation was received from Leo Chall for the President of the Society to participate in a new Society for the Advancement of Sociology which Dr. Chall proposes to form. The Executive Officer was instructed to thank Dr. Chall for the invitation but to state that the President of the American Sociological Society cannot in his official capacity accept such an invitation.

15. The Executive Officer was instructed to report to the sponsors of the *Journal of Educational Sociology* that the Executive Committee expressed interest in pursuing a plan for a publication in this field, and that, pending further negotiation, consideration is being given to the feasibility of starting a new journal. The Chairman of the Publications Committee was asked to discuss the matter with some of the leading experts in this field in order to make sure that suitable contributions and editorship would be available.

16. The Executive Office and the Secretary were instructed to explore possible sites, both hotel and university, for a 1961 annual meeting in the Midwest.

17. The Secretary and the Executive Officer were given full discretion in selecting a conference director for the 1960 annual meeting in New York City.

18. The Committee voted thanks to Marguerite Levy for her devoted and effective work in managing the Executive Office and assisting the Executive Officer.

Respectfully submitted,
DONALD YOUNG, Secretary

Report of the Delegate to the Third National Conference on Exchange of Persons

As the delegate of the American Sociological Society I attended and participated in the Third National Conference on Exchange of Persons held in Washington on January 28-31. The Conference was sponsored by the Institute of International Education in cooperation with 139 participating and observer organizations.

Over a thousand persons attended the plenary sessions, workshops, and clinics devoted to discussions of ways in which international understanding and relations can be improved through the exchange of artists, teachers, scholars, professional persons, students, and tourists. Special sessions were devoted to United States cultural relations with foreign nations, to exchanges with Eastern Europe and the Soviet Union, and to the relation of exchange programs to the national interest. One plenary session was addressed by President Eisenhower, who urged more "people-to-people diplomacy."

One workshop was devoted to educational exchange in the social sciences. Among the points emphasized were these: (1) there is an urgent need to train social scientists in an understanding of other cultures; (2) experience abroad should be utilized to test and to reformulate those social science theories and concepts which have been developed largely on the basis of Western conditions; (3) a better means of exchanging published social science materials between countries is needed, perhaps the development of international bibliographies, and certainly the greatly increased translation of materials; and (4) recruitment of social scientists must be improved. In connection with this last point, it was suggested that universities need to encourage the acceptance of appointments of some duration abroad by members of their staffs and to give these recognition rather than to adopt an "out-of-sight, out-of-mind" policy.

Among the many conclusions of the Conference a few were noticeable for the frequency with which they occurred in different sessions. There was agreement on the primary importance of bringing people of various countries together at the personal level to the maximum extent possible on the assumption that such relatively intimate contacts promote understanding between individuals and thus stable international relations. The exchange of artists and performers was judged to be especially constructive. It was emphasized, however, that persons sent to other countries, whether students, artists, experts, or others, and whether under private or public auspices, should always be the best available. The term "best" was defined to mean not simply the best qualified in a technical sense but well qualified persons who recognize their role as a representative of their own country and who will minimize the negative consequences of the "cultural arrogance" which some experts have displayed. Language training deficiencies and a lack of funds were recognized as two major handicaps to adequate exchange programs which must be overcome at least in

part. The Conference felt the exchange of senior professors to be meaningful only where the exchange involved a specific assignment in a foreign university or agency. It was held desirable to attract more men from other than a handful of major universities, in order to spread the benefits of international contact.

Conference memoranda noted that most American organizations giving grants or fellowships for foreign research or study include award programs in the social sciences. About 20 per cent of the Fulbright grants in 1956 were in this area. Some 25 per cent of American faculty members abroad in that year were social

scientists. By contrast, about ten per cent of foreign faculty members visiting in this country in 1956, and about 15 per cent of foreign students, were studying in some social science field. The consequences of these exchanges have been increasingly viewed as more than educational, particularly by sociologists, who have evaluated various aspects of the exchange process. A volume summarizing and integrating current and past research of this kind is under preparation by M. Brewster Smith under the auspices of the Social Science Research Council.

Respectfully submitted,
VINCENT H. WHITNEY

NEWS AND ANNOUNCEMENTS

NOTICE TO CONTRIBUTORS

to the *American Sociological Review*

Preparation of Articles, Research Reports, and Book Reviews

Papers are evaluated by the editors and other referees and are judged without author's name or institutional identification. Therefore, contributors are asked to attach a cover page giving the title, author's name, and institutional affiliation; the manuscript should bear only the title as a means of identification. Two or three copies should be submitted to enable prompt evaluation, but the author should retain a copy in his own files.

An abstract of about 100-125 words should accompany articles (but not research reports); it should present the principal substantive and methodological points.

Please prepare copy as follows:

1. All copy, including indented matter, should be typed *double spaced* on white standard paper. Lines should not exceed 5-6 inches.
2. A footnote to the title, author's name, or his affiliation should be starred (*). Other footnotes should be numbered serially, typed *double spaced*, and should be listed at the *end* of the article or research report. See current issues of the *Review* for footnote formats.
3. Each table should be typed on a separate page. Insert a guide line, e.g., "Table 1 about here," at the appropriate place in the manuscript. See current issues of the *Review* for tabular style.
4. Figures should be drawn on white paper with India ink and the original tracings or drawings should be retained by the author for direct transmission to the printer. Copies should accompany the manuscript.
5. Mathematical notation should be provided both in symbols and words. Explanatory notes not intended for printing should be encircled in pencil.
6. If any symbols are used that might confuse the printer, please clarify in the margin of the manuscript.

Preparation of News and Announcements

These columns may include notices of academic appointments, promotions, resignations, visiting professorships, leaves of absence, special awards, appointments to governmental and private organizations, new training programs and *major* curricular developments, special research projects and grants, special conferences and institutes, retirements, and deaths. *Do not include*: publications by department members, (these will appear in "Publications Received" and many will be reviewed), appointments to graduate assistantships, the conferral of graduate degrees (which are reported annually in *The American Journal of Sociology*) or of graduate work in progress, public lectures, televised courses, papers delivered at conferences, and brochures. Notices should be *concise*. See current issues of the *Review* for editing style. It is suggested that each department assign one person the responsibility of assembling and transmitting news and announcements.

Notices of professional interest from governmental and other non-academic agencies are welcome.

Foreign sociologists planning to visit the United States who are interested in meeting American sociologists are invited to send their itineraries and other pertinent details. These will be published under the heading, "Sociologists from Abroad." Please see *Time Schedule*.

The *Review* reserves the right to edit or exclude all items.

Time Schedule: To insure publication, announcements must be received no later than the beginning of the third month preceding the month of issue; e.g., to be included in the October issue, material must be received by July 1.

WILLIAM CHAPMAN BRADBURY, JR.
1915-1958

William Chapman Bradbury, Jr. and his wife Lorraine Patterson Bradbury were killed in an automobile accident on September 2, 1958. Two of their three children who accompanied them were spared. The tragedy occurred on their return from the Summer Session at the University of Washington, where Bradbury had been continuing his study of the Chinese language, under a renewed Ford Foundation Grant. Death thus disrupted his well-laid plan to consolidate his study of Leninist-Stalinist doctrine, and of the Soviet Union and the totalitarian social process, with his more recent researches on contemporary China. His study of Communist China was stimulated by his three years of work with data drawn from his interviews of Chinese and Korean prisoners of the Korean War—a task which he performed with distinction.

In William Bradbury's death sociology lost an excellent teacher and scholar. He received the B.A. degree, as did his wife, from Swarthmore, and the M.A. and Ph.D. from Columbia, where he was a University Fellow. He enjoyed a distinguished career as a teacher at Hobart College, 1939 to 1941, and at the University of Chicago, where he served in the College and in the Department of Sociology, from 1941 until his untimely death at the age of 43. An Associate Professor of Sociology and former Assistant Dean in the College, Bradbury was slated to head the social sciences faculty in the newly reorganized college.

He contributed significantly to the teaching program of the University of Chicago in helping to carve out a place for sociology in the general education curriculum, in participating in the action of the University's new administrative and functional organization for general education, and in stimulating large numbers of students as an unusually conscientious and effective teacher. As a scholar, Bradbury contributed importantly to research in inter-group relations, community organization, and political sociology. Unfortunately, much of his work is unpublished because it is in classified government reports. He carried his share of the load of training graduate students in sociology and

was especially helpful in his meticulous direction of student theses and dissertation research.

William Bradbury was a good citizen as well as a highly competent teacher and scholar. As a citizen he was dedicated to the task of helping to bring about a community of the free and equal. He worked diligently toward this goal on the international, national, municipal, and neighborhood levels. He was especially active in programs which not only enabled him to render important public service but, also, provided opportunity for research and contributions to sociological knowledge. Among other public duties, he served as Associate Director of Research of the National Committee on Segregation in the Nation's Capitol; as Consultant on intergroup relations and other problems to the United States Department of Interior, the Chicago Metropolitan Police, and the Atomic Energy Commission; and as a research scientist in the Psychological Warfare Division of the Human Resources Research Office at George Washington University. He served in the United States Army from 1943 to 1946. With the active cooperation of his wife Lorraine, Bradbury provided competent leadership to the Hyde Park-Kenwood Community urban renewal effort and especially to the resolution of problems of inter-group relations.

Those who knew the Bradburys will ever remember their capacity for friendship, and their unique combination of openness, vigor, and warmth. In their passing, we have lost, prematurely, fine human beings as well as a professional colleague at the prime of his career and at the threshold of a major cycle of development.

PHILIP M. HAUSER

University of Chicago

A Correction and Apology

The table of contents (p. 1) of the February, 1959, issue of the *Review* incorrectly lists "Roberta Roberts" as co-author of "Sociology in American Colleges: Fifteen Years Later." The author is Roberta Rogers (as noted on p. 87), to whom I owe thanks for calling my attention to this error and

my apology. My apology also to the readers. Recidivism, alas, is not confined to criminals. *The Editor.*

The Interamerican Society of Psychology announces that the Sixth Congress will be held August 16 to 21, 1959, in Rio de Janeiro, on the central theme, "Personality Evaluation and Human Relations." For further information address Dr. Samuel Pearlman, Executive Secretary for North America, One Hanson Place, Brooklyn 17, New York.

Universidad Nacional de Colombia has established a Department of Sociology. The new department is directed by Orlando Fals Borda, author of several studies of Latin American peasant societies, and recently appointed Director General of the Ministry of Agriculture of Colombia, who urgently requests readers and colleagues to send him sociological materials, reprints, duplicate copies, and used books towards building up the library. Address: Carrera 20, No. 54-38, Bogotá, Colombia.

American Academy of Arts and Sciences. Three Monograph Prizes of 1,000 dollars each will be awarded annually to the authors of especially meritorious unpublished monographs in the fields of the Humanities, Social Sciences, and Physical and Biological Sciences. The final date for receipt of manuscripts is October 1, 1959. For full details write to the Committee on Monograph Prizes, American Academy of Arts and Sciences, 280 Newton Street, Brookline Station, Boston 46, Massachusetts.

American Association for the Advancement of Science. Section K on Social and Economic Sciences will hold sessions for contributed papers at the annual meeting in Chicago, December 26 to 31, 1959. The American Sociological Society is an affiliate member of this section. Society members interested in presenting papers based on recently completed research should forward titles and abstracts not later than September 20 to Donald P. Ray, Secretary of AAAS, Section K, National Academy of Economics and Political Science, George Washington University, Washington 6, D.C.

Conference Board of Associated Research Councils, Committee on International Exchange of Persons. An expanded exchange program with Latin America has made possible the establishment of a limited number of short-term lectureships, an increased number of grants for Latin American scholars, and an increase in the number of regular lectureships supported under appropriations of the Smith-Mundt Act. Those interested in 1959 appointments should write immediately to the Committee, 2101 Constitution Avenue, N. W., Washington 25, D.C.

The Hartford Seminary Foundation. A new program designed to train professional sociologists for research work in church-related agencies is being offered. Inquiries should be addressed to

Peter L. Berger, Director, Institute of Church Social Service, The Hartford Seminary Foundation, 55 Elizabeth Street, Hartford 5, Connecticut.

The Michigan Sociological Society met as the Sociology Section of the Michigan Academy, Arts and Letters, at Michigan State University on March 27.

National Council on Family Relations. The Annual meeting will be held on August 19 to 21 at Iowa State College. Aaron Rutledge, Program Chairman, announces that the theme will be "Growing Individual Values Within the Family." For information write to the National Council on Family Relations, 1219 University Avenue, S.E., Minneapolis 14, Minnesota.

The Society for the Scientific Study of Religion. The decennial meeting, held at the University of Chicago on April 10 and 11, included the following participants: William Kolb, Tulane University; Howard Becker, The University of Wisconsin; O. Hobart Mowrer, University of Illinois; James Luther Adams, Harvard Divinity School; and Milton Singer, The University of Chicago.

A fall meeting will be held on October 30 and 31 at Yale University. Social scientists who wish to contribute reports of empirical studies or problems in the field of religion should write to Dr. James E. Dittes, Chairman of the Program Committee, Yale Divinity School, or to Dr. Walter Houston Clark, Secretary-Treasurer, Hartford Seminary Foundation, Hartford 5, Connecticut, no later than June 15.

The Society for the Scientific Study of Sex will hold its second annual meeting at the Barbizon Plaza Hotel in New York on November 7, 1959. The meeting will include special symposia on "The Psychological Aspects of Infertility" and "What is Sexually Normal?" For further details contact Robert V. Sherwin, Executive Secretary, Suite 704, 1 East 42 Street, New York 17, New York.

Boston University. A new research program designed to relate the arts to current and emerging social and technological conditions has been announced, conducted by The Arts Center as part of the School of Fine and Applied Arts. The Center has organized an inter-disciplinary bi-monthly seminar, plans publications, and hopes to obtain grants to establish Fellowships for graduate studies. For further information, address Max Kaplan, Director, The Arts Center, Boston University.

Bucknell University. Beginning with the 1958 term, the Department of Sociology includes Professor Ralph Spielman, Chairman, William F. Byron, Visiting Lecturer, and Helmut R. Wagner, Associate Professor.

University of California, Los Angeles. Donald R. Cressey, Chairman of the Department, has been elected President of the Pacific Sociological Society for 1959-1960. Wendell Bell will be on leave during the 1959 spring semester to continue studies on political change, leadership, and social mobility in Jamaica under the final year of his three-year

Social Science Research Council Faculty Research Fellowship. Oscar Grusky has been awarded a Ford Foundation Faculty Fellowship in Social Science and Business for 1959-1960. Richard Hill is directing a research project designed to evaluate adult education programs, under a grant from the Ford Foundation Fund for Adult Education. Franz Adler, from the University of Arkansas, is serving as Lecturer in Sociology in 1958-1959, replacing Svend Riemer, who is on sabbatical leave to conduct research on housing in Europe.

Dickinson College. "The Social Sciences—A Journal of Student Research," a new journal designed specifically to broaden the outlet for student research in the social sciences, will begin publication shortly. Articles not exceeding 2,000 words may be submitted to Professor Edward Rothstein, Department of Sociology, Dickinson College, Carlisle, Pennsylvania.

University of Florida. T. Lynn Smith has been appointed as Graduate Research Professor of Sociology, a newly established rank at the University.

Hamline University. Julianna Schmidt has received a Fulbright Exchange Fellowship and will lecture in social work in Thailand for one year, beginning in June, 1959.

University of Michigan. A behavioral science computer newsletter has been initiated as a department of the quarterly journal, *Behavioral Science*, published by the Mental Health Research Institute. Manuscripts concerning computer systems, programming, and other related topics may be sent to Dr. Steven G. Vandenberg of the Institute.

Michigan State University. John Useem has returned from a sabbatical year in India, during which he and Ruth Useem engaged in a field study of Americans in cross-cultural relations. Their research was sponsored by the Hazen Foundation.

Paul Miller has been appointed Vice President of the University in charge of off-campus education.

Jay Artis has been appointed Director of the laboratory of the Social Research Service.

William Form and Charles Loomis, under collaborative arrangements with the University of Texas, and with support from The Committee on Disaster Studies of the National Research Council and the Carnegie Corporation, are directing a study of the social aspects of the recent Rio Grande flood. Harry Moore represents the University of Texas, and Ellwyn Stoddard and Arturo de Hoyos of Michigan State University did the field work in Abasco, Texas and Reynosa, Mexico.

Baron Moots of the University of Michigan has joined the staff. He has a joint appointment in the Department and in the Institute for Community Development and Services.

Presbyterian-St. Luke's Hospital, Chicago has received a grant from the Commonwealth Fund to study subjective aspects of patient care. Under this grant a Department of Patient Care Research

is established which will serve as nucleus for further research activities. Hans O. Mauksch has been appointed Director while maintaining his position as Chairman in the Social Science Department of the School of Nursing. The latter Department is continuing its part in the experimental program. A study is in preparation to evaluate the impact of a strong social science program on the nurse. The Department now includes: Wolf Heydebrand, Otto Schlesinger, John Sims, and Charles Van Buskirk.

University of Texas Medical Branch, Galveston. Sam Schulman has resigned to become Director of the Rural Health Survey in Santa Fe, New Mexico, to investigate health conditions and attitudes among the Spanish-speaking peoples of northern New Mexico. In his place, S. Dale McLemore has been appointed Assistant Professor of Medical Sociology.

E. Garity Jaco will direct a project on "The Effects of Cultural Differences and Experimentally Induced Ward Structures on Therapeutic Outcome" at the Cleveland Psychiatric Institute and Hospital in June, and hold a visiting appointment in the Departments of Sociology and Psychiatry at Western Reserve University at the same time.

Texas Technological College. Beginning September 1, 1959, there will be a separate Department of Sociology in the School of Arts and Sciences. The staff of the Department will include W. G. Steglich, Professor and acting head; Earl Lomon Koos, Professor; R. Sylvan Dunn and Julius Rivera, Assistant Professors; and Pauline Bowers, Instructor.

Koos, Rivera, and Steglich are engaged in making a demographic and ecological analysis of the minority populations of Lubbock on a grant from the Texas Technological College Research Foundation.

University of Toledo. Janina Adamczyk, Chairman of the Department of Sociology since 1945, has resigned that position for reasons of health. She continues in the Department as a Professor. James B. McKee has been appointed Chairman of the Department.

Wayne State University. Edgar A. Schuler has returned from a year at Thammasat University in Bangkok on a Fulbright Research Grant, resuming his duties as Departmental Chairman.

John Biesanz is on sabbatical leave as Fulbright Lecturer in Sociology at the Pedagogische Hochschule, Frankfurt-am-Main, Germany.

James B. Christenson has been granted a Senior Postdoctoral Fellowship by the National Science Foundation for ethnographic research among the Bisa of Northern Rhodesia, beginning July of 1959.

Frank E. Hartung returned from sabbatical leave in February, 1959.

Gabriel and Bernice Kaplan Lasker have returned from Peru, where they conducted a series of village studies.

Alvin W. Rose was elected to the Board of Directors of the Michigan Society for Mental Health, Inc., to serve a three-year term.

REVIEW ARTICLE

"MODELS OF MAN" *

FREDERICK MOSTELLER

Harvard University

This, the first book of its kind, introduces sociologists and political scientists to mathematical models and thinking in areas of their own substantive interests. The work is intended as "a consistent body of theory of the rational and nonrational aspects of human behavior in a social setting" (p. vii). Since the sixteen papers included were published in thirteen journals, their coherence could not have been appreciated until they were brought together.

Simon invents a sequence of mathematical models to describe and to interpret social phenomena. The range of social problems is broad, and the mathematics used is varied. The four parts (I. Causation and Influence Relations, II. Social Processes, III. Motivation: Inducements and Contributions, IV. Rationality and Administrative Decision Making) of the book are loosely laced together by four new essays that show the interrelations between papers and between parts.

If these essays on sociology, political science, philosophy, economics, administration, and statistics are viewed as a whole, we find that "all of them are concerned with laying foundations for a science of man that will accommodate comfortably his dual nature as a social and as a rational animal" (p. vii).

The decision to collect the essays in their original form instead of writing a systematic treatise was deliberate; Simon seems to think the reader can then explore unfamiliar ground from a familiar home base (p. viii). To write a systematic treatise based upon this material might take all a man's life, I think, but such an effort would be premature. No doubt, too, the decision is based partly on the author's own theory of "satisficing": essentially, this is good enough for this moment, considering the cost of an integrated job, and considering alternative contributions the author can make.

* *Models of Man: Social and Rational. Mathematical Essays on Rational Human Behavior in a Social Setting.* By Herbert A. Simon. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1957. xiv, 287 pp. \$5.00.

PART I. CAUSATION AND INFLUENCE RELATIONS

The author says that the keynote of Part I is *asymmetrical relations*: if A has power over B, we don't necessarily mean B has power over A. Casuality has just such asymmetry, and the first paper deals with causal ordering. Since this is an extremely important paper, I wish to explain the idea in some detail through an example. Suppose we have two simultaneous linear equations in two unknowns x and y :

$$\begin{aligned} 3x &= 6 \\ 2x + y &= 5. \end{aligned}$$

If you secretly have only the first equation and I only the second, you can find the value of the unknown x in your equation, but I can make no progress toward selecting the appropriate value of y until your solution is supplied. In a sense, then, the knowledge of the value of x is prior to a knowledge of the value of y . Or at least the solution of the first equation has a sort of precedence over the solution of the second.

Simon considers more complex situations—ones in which each linear equation has a double-barrelled meaning. Thus the equation $3x = 6$ may stand for an experiment in which a block of length x is laid beside itself 3 times to produce a total length of 6. The second equation $2x + y = 5$ may have a similar meaning for two blocks of wood, our old one of length x and a new one of length y .

The first of these experiments has precedence over the second. We do not mean that the size of the first block somehow changes the size of the second, but that while the outcome of the first experiment is unaffected by the outcome of the second, and the first experiment is analyzable by itself, a satisfactory analysis of the second experiment must wait for the first, and the second experiment alone can determine the value neither of x nor of y .

Naturally situations exist with two variables where neither variable has "precedence" over the other (for example, the equations and experiments described by $2x = 4$, $5y = 1$). Simon's whole approach leads to a causal-ordering clas-

sification of systems of simultaneous linear equations in two or more unknowns. Applications to economics are discussed.

Two criticisms may be made at this point. First, systems of linear equations aren't very general, and second, there may not be a unique representation of the experimental or observational system in the appropriate manner. While I have been concerned about the second matter, I have been unable to settle it to my satisfaction. For the first, I would point out that a great deal of the world's science is built upon linear systems at least as approximations, and further, that Chapter 3 attacks the more general problem from the point of view of symbolic logic. However, experts tell me that the degree of success achieved in Chapter 3 is not clear.

In spite of any trouble about Chapter 3, Chapter 2 deals with spurious and non-spurious correlation from the point of view of causation, and essentially from the point of view developed in Chapter 1. For simplicity, the author restricts the discussion to three-variable systems. To decide whether a partial correlation is or is not spurious (does not or does indicate a causal ordering), we must have made *a priori* assumptions that certain other causal relations do *not* hold. We get some of these assumptions from "common sense." (Sometimes precedence in time supplies these assumptions.) For example, let *x*, *y*, and *z* be measures of candy consumption, marital status, and chronological age, respectively. We are prepared to assume that *z* is not causally dependent on *x* or on *y*. Statistical assumptions form another helpful set, for example, the assumption that certain residuals are uncorrelated. Together, the two kinds of assumptions are enough to imply certain causal orderings in the problems treated here.

The tie-in with Chapter 1 is strong and direct, and the analysis of causal implications striking for correlation studies. (The reader may wonder how he can know that the system is self-contained, but that's another story.) Kendall and Lazarsfeld have treated still other problems of this type.

Chapter 4 is called "Notes on the Observation and Measurement of Political Power." The reader's guide tells us that the author returns here to the problem of power and applies to it the methods of Chapters 1 and 3.

This long article repeats the asymmetry notions, worries about anticipated reactions and about cardinal *versus* ordinal numbers for measuring power, but never does anything about power. There is no direct application of the ideas in Chapters 1 or 3 that could not have been made had these chapters been stillborn. It is hard to

believe that the ideas presented are not very well known. For example: "The position taken here is that the phenomenon we wish to measure is an asymmetrical relation between the behavior of two persons. We wish to observe how a change in the behavior of one (the influencer) alters the behavior of the other (the influencee)" (pp. 77-78). And the summary on units for measuring power and influence: "Our principal conclusion here is that we must be prepared to admit into our measurement schemes many other kinds of units besides cardinal numbers. In particular, certain notions from set theory, such as the concept of partial ordering among sets, may be suggestive of fruitful schemes of measurement" (p. 78).

I think that Chapter 4 is a sort of "get-ready" article, or a "let's-all-think-hard-about-this" article. It may have helped to organize Simon's thoughts, but for many readers the ground must be pretty well worn except possibly for the discussion of measurement which says in effect, problems with many facets often do not have simple one-dimensional answers. And the remarks about partial ordering may not be saying any more than this: "In many cases we shall have to be satisfied with the kinds of descriptions and comparisons that historians and political scientists ordinarily use." Instead of a think-piece what is needed is an illustration of the measurement of political power employing the ideas of the earlier chapters.

Chapter 5 on "Bandwagon and Underdog Effects of Election Predictions" has the summary statement (p. 79): "Social research has often been attacked on the grounds that the research itself so altered the original situations as to make accurate predictions impossible. In this article, the author deals particularly with the effects of published predictions and the adjustments necessary to account for reactions to those predictions."

It turns out that if for each possible condition of the population (say each possible per cent Republican) it is known what the voting behavior will be after every possible election prediction, and if the behavior is a smooth function of the prediction, then a perfect forecast can be made. The point is that you don't tell the data gathered (which are exactly the truth about the population if the forecast is private) but choose an improved forecast based upon them, *taking account* of bandwagon or underdog effects. It is doubtful that many social scientists regard this as brave news. I have asked six what they thought. I was surprised to discover that only one of these had thought of the self-falsifying prophecy problem in this way. They got tangled up in the iterative approach—"I'd add a little to the

prophecy, but then I'd still undershoot, so I'd add a little more, etc." So there was a point to be made by the article. But when told the answer and what had to be known in order to handle it, they felt pretty discouraged: "Who could know all this?" My sociological friends have little enthusiasm for this type of existence proof. The justification for Chapter 5 is that if forecasts are self-defeating, then we can't have any behavioral science. This problem is an old chestnut that any mathematician would enjoy solving, just as he enjoys pointing out that "the greatest good for the greatest number" is not a workable principle. But its solution won't cause a ripple today. Still, it only took a few pages.

PART II. SOCIAL PROCESSES

Though it's risky to say what will interest a sociologist most, Part II bears directly on his interests.

The first paper deals with interaction in social groups. It develops the now well-known Simon-Homans theory. Simon translates Homans' variables into a system of differential equations and then considers the relations among intensity of interaction, level of friendliness, amount of activity within a group, and the amount of activity imposed by the external system. Simon's equations capture much of the information contained in Homans' propositions, and they especially offer an explanation of phenomena related to the stability and dissolution of groups.

As I understand it, the purpose of mathematical models of this kind is to wrap up otherwise isolated pieces of information into a dynamic system and thereby provide a better understanding of the system, including an exploration of limiting conditions. To use the model to make predictions about the future course of a given system may be difficult, but one can comment about how such systems change with time. The measurement problem is not touched here; possibly it never will be—the model is geared more to understanding than to prediction, except in gross, but important, ways.

The next two chapters deal with pressures toward uniformity in groups. They were written jointly with Harold Guetzkow (to whom the book is dedicated). These chapters develop systems of differential equations that form models for some of the work of Festinger and his colleagues.

In the Homans system it is assumed that (1) friendliness increases interaction and (2) vice versa. In the Festinger system it is assumed that (3) cohesiveness produces uniformity of opinion (via pressure to communicate) and (4) vice versa. Elsewhere it is assumed that (5) interaction produces uniformity. If we regard

cohesiveness and friendliness as synonymous (p. 95), then the variables are cohesiveness, interaction, and uniformity of opinion, and they turn out to be related through the five mechanisms listed above. The grave need for rewriting was never clearer than here. In the introduction to this part the author implies that he now has an overarching theory that connects these three papers (plus a fourth) into one coherent whole, but we are only given a diagram of the ideas (on page 96).

The model developed in Chapter 7 is checked as far as possible against an experiment by Back (in which two subjects who had seen somewhat different pictures discussed them under the assumption that both had the same information), an experiment by Festinger and Thibaut, and a housing study by Festinger, Schachter, and Back. The results do not lend themselves to ready summary here, but sociologists should find them instructive.

A second work (Chapter 8), again related to Festinger's, is concerned with a group with a deviate member. The construction of the model made possible a check of the assumptions, but suggested that some assumptions made by Festinger and his associates were not being tested in the experiments. Two variables formerly regarded as independent were found to be interpretable as opposite ends of a continuum (cohesiveness and rejection). The model for a deviate member and a model for groups without deviates were found to be special cases of a single larger model.

Chapter 9—"On a Class of Skew Distribution Functions"—has special interest for me because I have long had an interest in collecting what I call "abnormal laws of large numbers." Zipf's Law is one of these. Examples include distributions of words in prose by their frequency of occurrence, distribution of cities by size, and distribution of scientists by number of papers published. These distributions can be approximated by

$$f(i) = ab^i/i^k, \quad i = 1, 2, \dots,$$

where $f(i)$ is the frequency of things that have occurred i times, and a , b , and k are constants. For example, the number of words that appear exactly i times in Joyce's novel *Ulysses* is roughly of the form a/i^k .

The author develops a couple of stochastic models that yield Zipf's Law (Simon suggests calling it Yule's distribution because of priority). A model is first developed for word counts in prose and then considered for four other areas. In the model for words, Simon assumes that after many words have been written in a work, the probability of the occurrence of a brand new word is a constant α , and that the probability

of a word that has already occurred i times (given that an old word appears) is equal to $\frac{if(i)}{\sum_j f(j)}$ where $f(i)$ is the number of words that

have occurred i times up to this instant. For example, if somehow we have 11 words that appeared once, 3 that appeared twice, and one that occurred 3 times, then if the next word is an old word, the probabilities are 11/20, 6/20, and 3/20 respectively that it is a word that has occurred once, twice, or thrice. After the word appears probabilities are readjusted. The theory (asymptotic) associated with such a process is explored, and found to yield functions that fit decently the known results for several examples studied. No attempt was made to study internal consistency. (Without meaning

1. The O[rganisation]-theory is concerned primarily with the conditions under which an organisation will survive; since survival does not in general determine uniquely the distribution of satisfactions among the participants, it provides no answer to the question of distribution.

The traditional F[irm]-theory selects out of all the viable solutions that one which maximises in a certain sense the satisfaction of the entrepreneur. It assumes that he alone will seek to capture the "surplus" of satisfactions, and that the other participants will be persuaded to remain in the system by (marginal) inducements that just equal the (marginal) opportunity costs of their respective contributions.

2. In the O-theory it is assumed that the participants will make the all-or-none choice of participation or non-participation. In the F-theory it is usually assumed (remembering that we exclude theories of oligopoly and monopoly-monopsony) that there will be bargaining for the marginal unit of inducement and contribution.

ESTIMATE OF α (PROBABILITY OF A NEW WORD)

Word number	1	5	10	15	20	25	30	35	40	45
Tam O'the Scoots	1.00	.970	.938	.856	.818	.786	.764	.742	.730	.725
Mystery of the Frightened Lady	1.00	.972	.938	.882						

to be overcritical, I would warn against assuming that if a model generates a satisfactory answer, then the model describes the process. The trouble is that in stochastic processes, more than in some other areas of applied mathematics, quite different processes can lead to the same results.)

I was bothered by the assumption that the probability of a new word was a constant (Simon was too, and explored the problem for a slowly decreasing α). It seemed to me that there would be a reverse curve, essentially the opposite of an ogive, high at first and falling slowly, followed by a point of inflection. We are talking now about a more local phenomenon than Simon intends to deal with. I found it difficult to invent a model that generated such results, so with the help of Robert L. Conrod, Lindsey Churchill, and Cleo Youtz, I ran a few experiments to estimate α for two writings of Edgar Wallace. The results may interest readers. (See table above.)

When graphed up, these results do show that α starts out very flatly at the beginning, drops, has an inflection point, and then levels off slowly. We have not carried the matter far enough to check the asymptotic behavior of α . Even the short investigation reported here is a time-consuming job.

PART III. MOTIVATION: INDUCEMENTS AND CONTRIBUTIONS

Chapter 10 compares a theory of the firm with a theory of organization. Briefly (p. 174):

The author develops some mathematics to show that in some situations the two theories may agree in their results, in others not.

Chapter 11, "A Formal Theory of the Employment Relation," states that a worker has an employment contract if he agrees to accept the authority of the employer for a stated wage. He has a sales contract if he agrees to perform specific services for a specific consideration. The author develops models to explain why both such contracts may be in use. And he finds that under certain ideal circumstances the employment contract would always be preferred by both parties, but for this to be true the worker must be confident that the employer will act in a long-run rational manner rather than a short-run one that the employer has "enlightened self-interest."

PART IV. RATIONALITY AND ADMINISTRATIVE DECISION MAKING

The essay introducing this part of the book seems outstanding to me. It discusses the difficulties for human beings of basing their behavior on any notion of strict rationality. Though logic itself can be a part of a theory of behavior, it is questionable whether such a theory will satisfactorily explain most behavior. The usual "rational man" seems to need to know too much, to understand too well the consequences of his acts. The game theory approach and L. J. Savage's approach both require the sort of person who doesn't make mistakes. There aren't many people like that left.

What we need is a theory that does not require omniscience or unlimited computational power, argues Simon. I suppose one way to get such a theory is to introduce costs for doing logical problems and costs for the acquisition of information. The latter is done in statistical decision theory.

Simon proposes instead that we replace the goal of maximizing with that of "good enough"—what he calls "satisficing." If I can win a chess game, that's enough; I don't have to have the most elegant win. This is not quite a doctrine of mediocrity since an aspiration level can be set quite high, so high that the satisficing is impossible, if the aspiration level is higher than the maximum achievable.

The first paper, an equilibrium analysis, explains why it makes sense to leave the country for the city (why our urban-rural population ratio increases); probably neither the methods nor the results are particularly unusual. The second shows how the theory of a servo-mechanism can be applied to the control of production.

Chapter 14—"A Behavioral Model of Rational Choice"—is a good article for the person who has had no previous experience in this area and who wants to know a bit about it. He will quite quickly get hold of the concepts that are in common use. The paper does not really give a model; it is another "get-ready," "let's-think-about-this," "here-are-the-concepts-we-might-work-with" paper. Chapter 15 shows how a rather limited organism can have a good chance of surviving in a world with unlimited, but not automatically accessible, resources if only he can look ahead a little, locomote, and store up a little food. The example is especially illustrative of the gains available from looking a little way ahead. Persons discussing the need to look

ahead in chess games usually do not appreciate the strength of a player who looks as little as, say, two moves ahead. One thinks one knows how this player behaves, but one forgets that he may change his mind at every move. Thus seeing only two moves ahead but moving one move at a time is a far cry from seeing two moves ahead and having to decide to make both those moves before new information may be used.

The final paper offers a rationalization, through some game theory work, of apparently non-rational behavior by subjects in certain psychological experiments. Saul Sternberg has called my attention to an error in the interpretation of Simon's model as that of Estes. (The error does not seem to be important if the subject is little affected by the outcome on a single trial.)

In Part IV the emphasis on limited or bounded rationality forecasts the later interest in and development of the artificial intelligence idea that Simon and his associates have done so much to encourage.

Some books are for leafing through and throwing away, some permanently for the shelves. This one is neither of these. It is worth keeping until its natural successors come along. This cannot be immediately because the necessary research is yet to be done, and because the author has not yet chosen to integrate what he has done.

I have indicated that I found the book stimulating. Some sociologists have found, and others will find, this book to be a good foundation for an exciting graduate seminar. Moreover they may find in it a new and special reason for establishing relations with a member of the Mathematics Department, namely, common research interests.

BOOK REVIEWS

Men and Their Work. By EVERETT CHERRINGTON HUGHES. Glencoe, Ill.: Free Press, 1958. 184 pp. \$4.00.

This is a collection of thirteen of Professor Hughes' best essays on work. About half are known to sociologists through reprint or frequent citation ("Dilemmas and Contradictions of Status," "Institutional Office and the Person," "Work and the Self," "Mistakes at Work"); half are new or known only to small audiences ("License and Mandate," "Professions in Transition," "Cycles, Turning Points and Careers," "Social Role and the Division of Labor," "The Making of a Physician"). That much of the book will seem familiar to specialists is a measure of how widely Hughes' ideas have spread—directly or by text and student propagation. The book represents three decades of writing and thinking about the organization of work and the nature of work experience. Industrial sociologists will welcome it as a demonstration that close observation in an empirical area can illuminate theoretical problems at the core of the discipline.

The "core problems of sociology," says the author, are "all the processes involved in the definition and enforcement of moral rule" (p. 101). He sees his essays as dealing with "the struggle of the individual to find a place and an identity in the world of work" and "the collective efforts of occupations to exert control over the terms of their work." He comments that these are "the social psychological, rather than . . . the organizational aspects of work." Yet, what holds the essays together is a sustained concern with the process of the division of labor and the problem of social control in and of occupational groups. If Hughes deals with the person, it is always with the aim of understanding social structure: ". . . the career of a man is worked out in some organized system without reference to which it cannot be described . . . the career of an occupation consists of changes of its internal organization and of its place in the division of labor of which society itself consists" (pp. 8-9). Hughes speaks of roles, but this is not "role theory" of a social psychological kind. In fact, this book can help us remember what is crucial about roles: they have a history, become instituted in systems (often for a purpose), serve functions in larger social contexts, and undergo changes as these con-

texts change. With Hughes we must grasp not merely Henderson's "physician and patient as a social system" or the mutual expectations of school superintendent and board member, but an entire institutional matrix. Whether the author is describing medical specialization or stages in individual careers, he remains alert to "lines along which investigation about occupations becomes investigation of society itself."

Students of modern society are inclined to think that when a man gives us his attitude toward his immediate work situation we have hold of something momentous. Hughes' perspective is a healthy corrective, for he is keenly sensitive to the flow of time—to change and yet continuity. When he deals with careers it is in the broadest sense of "the moving perspective in which persons orient themselves with reference to the social order, and of the typical sequences and concatenations of office," or as sets of projections of self into a partially unknown future, a "running, never-completed adjustment" (pp. 129-130). In all, when the person is his unit of analysis, he reminds us of interlocking cycles of career, consumption, and family—one of many research leads which have been too little noticed. On the organizational side he emphasizes the collective mobility of occupations, giving us a neat statement on stages in the development of a profession and a discussion of the impact of "technical development, social movements and/or new social institutions" on conflict and cooperation among colleagues, clients, and occupational groups.

This is a book with the flash of insight. On the universality of standards of work beyond successful conclusion of the task: "The quack, defined functionally and not in evaluative terms, is the man who continues through time to please his customers but not his colleagues" (p. 98). On occupations requiring guilty knowledge: "The priest cannot mete out penance without becoming an expert in sin." On professionalism: "In the purest case the professional would do work which he alone can do, and the work would be of a kind wanted everywhere by all men; a maximum of specific bounding would be matched by a maximum of universality" (p. 162). However, this model applies less well to societies at lower levels of economic development. "Many of our ideas [about professionalization] . . . rest upon the assumption of . . . a large market" (p. 165). On stages in careers

(why physicians do not like their colleagues to become consultants too early): "Consulting is something for the crowning, easing-off years of a career; something to intervene briefly between high power and high blood-pressure" (p. 107).

The whole is inspired by a creative and deliberate naiveté—a resistance to received labels which permits Hughes to bring the widest range of cases into view. We can lump together: plumber and physician (their routine of work is comprised of the emergencies of others); school-room, clinic, operating room, confessional booth, and undertaking parlor (a structured temptation for the service functionary to over-use discipline); physician's handling of the body and janitor's removal of garbage (the "dirty work" common to all occupations, sloughed off if the occupation moves up)—and, finally, we can borrow a phrase from the labor economist and see all groups as bent toward job control. This comparative approach is combined with other useful strictures on method: beware the public relations facade of the highly-placed, "start study of any social phenomenon at the point of least prestige" where processes ordinarily hidden come more readily to view; be disinterested, search for the timeless, but let your curiosity roam, and, in the Cooley-Park tradition, put yourself in intimate touch with the people and problems under study. The book concludes with some timely comments about the costs of premature professionalism in sociology.

One can appreciate all this and yet feel restive. It is so casual. One wishes that an essay like "Personality Types and the Division of Labor" (1928, somewhat revised), had been elaborated in light of the work Hughes and others have been doing in the years subsequent. We read a two-page classification of occupational roles in which the following criteria are invoked: conditions of entry, attitudes of incumbents, community prestige, dependence upon stable personal contacts, colleague relations, and competition. The classification itself, however—missions, professions or near-professions, enterprises, arts, trades, jobs—rests almost entirely upon methods of entry and recruitment; the other variables get lost (pp. 32-34). Similarly, Hughes and his students write perceptively of "stages" in careers. But the hard task of systematic classification of careers by number, duration, discreteness, and other attributes of the stages, and the disciplined elaboration of the relations among attributes of careers or between career types and other aspects of the organization of work—these remain to be done. It is true that unless we begin with something like these essays, with their steady focus on

social order and social change, we are more vulnerable to theorizing that bears only a dim relation to the sociological mission and the facts of occupational life. A general sociological orientation such as that of Hughes, is indispensable; it cannot substitute for theory.

These papers have originated or enriched many studies (for example, much of the sociological description of occupational cultures derives from them); they have also guided curriculum planners, to whom courses in occupations and professions no longer seem unnatural. The ready availability of the essays should do even more to stimulate interest in their subject. In a sense, however, their chief strengths are also their undoing. They are like too many "exploratory" cocktails before dinner. We are led on from one to another, appreciating each more than the last; the encounter with solid food is thereby not only delayed—our taste for it is dulled. And for the further development of the sociology of work, that would be too bad.

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The Institutions of Advanced Societies. Edited by ARNOLD M. ROSE. Minneapolis: University of Minnesota Press, 1958. ix, 691 pp. \$10.50.

The editor of this volume has the best of intentions in this field. He feels that we have few adequate teaching materials in comparative sociology, that we are particularly deficient in those on so-called advanced societies, and that the students and the field would be best served if he could get essays by able men on different societies in which the authors themselves lived. There may be some who would quarrel about the last of his three points, but on the whole these intentions will excite nothing but a chorus of "Amen." To carry out his objectives Professor Rose has selected the United Kingdom, Australia, Finland, Poland, Yugoslavia, Greece, Israel, France, Brazil, and the United States as vehicles. He would have liked to add at least one large scale advanced Asian society but was unable to find a scholar who filled his bill. Almost anyone would find similar difficulty. Less satisfactory is our understanding of why these societies and not others such as Holland, Belgium, Denmark, Norway, Spain, Italy, or Portugal were chosen. Any list would raise problems. One may wish that we knew more of the reasoning that lay behind the selection, but the choice does not pose the major problem of the work.

Professor Rose introduces the volume with an essay on "The Comparative Study of Insti-

tutions." I can not for the life of me decide whether this chapter is primarily designed to communicate with laymen and introductory students or represents the editor's mature views for the circle of his peers. I incline to the former hypothesis when I read his attack on functionalism, with or without its follies (pp. 13-23), and when I read what seem to me to be oversimplifications about sociology in general. (See p. 15 on which we learn that the major task of sociologists "... is not to describe the modal behavior of a society or subsociety but rather to account for variations in behavior.") I incline toward the latter hypothesis when he sets forth the theoretical framework which his authors in greater or lesser degree have tried to apply (pp. 23-29).

The individual essays are, on the whole, pleasant. In almost every case the strongest point is the presentation of historical background as a basis for the discussion. None of the essays seems to get very far as a general description and analysis of the society concerned. As most of the authors point out, there are very serious obstacles to this end. The worst of these is one with which we are all familiar: it is exceedingly difficult in any scientific field to write a communicative essay for the layman or elementary student in advance of the basic research work presumed to underlie such efforts. One can hardly blame either the editor or the authors for this. In general, the pressures placed on social scientists by themselves and others incline the victims to prefer communication to knowledge if one is so unfortunate as to have to make a choice between the two.

One might have guessed that the essay on France would be the most urbane and witty (even in translation), those on the United States and the United Kingdom best backed up by empirical studies, and that on Israel best oriented to the role of a single problem in shaping the fate of the society (and hence in this case most fully discussed in terms of interrelationships within the society). The essay on France contains one of the few serious and interesting contradictions to common sense in the entire volume, when it refers to the non-conservatism of French peasants (p. 467).

I cannot see exactly what any of the essays has gained from the common framework tendered the authors by the editor. This may on the whole be fortunate. Since the large scale basic research on these matters has not been done, I don't quite know how the scheme could be applied even were it developed at the requisite level of generality and systematization. Fortunately the scheme itself has not been de-

veloped to any point that would allow it to impinge upon the originality and taste of the authors. This is particularly marked in Professor Bourricaud's essay on France which makes delightful use of literary works as source material. I am not sure that French society is thoroughly pictured thereby but one gets a wonderful sense of what some of France's most brilliant authors thought France to be. Was Stendhal right about French individualism, love, women and their roles? If so, when and where, temporally, geographically, and socially? What is one to do with those societies in which the wealth of social materials is literary rather than statistical?

The clearest problem posed by the system of analysis and the editing is raised by the case of Yugoslavia. The author of this essay holds an official position in Yugoslavia, and may well have a care for what he says. Even granted that it ill behooves us from more fortunate settings to criticize, still the portrayal of Yugoslavia as a system wherein, by 1953, there had been "a complete democratization of government" (p. 315) is a bit thick. If this is an idealized and politically expeditious picture rather than the product of a new kind of sociological analysis, a discreet editorial note advising us of this fact would seem to be called for. I say this only as a matter of principle since in practice I do not think the presentation is likely to mislead either critical undergraduates or laymen, however unfamiliar they may be with sociology.

As this volume is presented, I think it promises us bread and gives us stones. As a set of essays on selected societies, however, quite another judgment is in order. As brief essays on some aspects of these societies they are on the whole admirable, especially in their brief developments of historical settings. If one's interests happen to focus on this particular set of societies, or even on a single one of them, I cannot think of a better brief starting point.

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The Population of Japan. By IRENE TAEUBER. Princeton: Princeton University Press, 1958. xv, 461 pp. \$15.00.

In his Foreword, Dr. Notestein calls this "a work that we think will prove to be a landmark of demographic analysis." While innocent of the subtleties of that field, your reviewer on other grounds fully shares Dr. Notestein's enthusiasm. *The Population of Japan* is a superb piece of scholarship, and a model of meaningful research. In content, it is the only authoritative and comprehensive professional study of its topic in a Western language. But it is also very

much more: a quest for understanding of the relation among changes in population, culture, and modernization. It most certainly ought to become a landmark in the comparative study of the demographic transition, for it is the first definitive attack upon the problem for a non-Western area. It will also, however, stand as a remarkable study of the interrelation of population with social and cultural factors, and, for the area specialist, as an unparalleled demonstration of certain important features of Japanese life on the widest national scale.

There are chapters (including some projections) on the following: a survey of Japan's population history through 1955; special studies of the working population, the family, internal migration, industrialization, and urbanization; the local problems of external migration, World War II demography, and population policy; analyses of marriage, fertility and its control, mortality, and natural increase. The data are the reward of years of amicable cooperation between Dr. Taeuber and her Japanese opposite numbers, and so includes the best they have to offer plus many tabulations to which her own sense of problem has led. Backgrounds and relevant local practice are explained fully enough so that a non-specialist in either demography or Japan can follow with comprehension. The extensive bibliography of both interpretative and statistical publications lacks a few important sociological items, but this does not enfeeble Dr. Taeuber's treatment of culture and society in the text. Not the least of its merits, the demonstration has the discipline of a soldier's drill: beginning with succinct statements of the problem, chapters lead through protean and often ingenious evidence without ever losing sight of their purpose.

Basically, of course, Dr. Taeuber fully documents our impression that in its broad lines the demographic transition in Japan has been about the same as in the West. Modernization has brought about a marked population increase (of 200 per cent during the last century), to date covered by an even greater economic advance as gauged by all indices. In the early period, much of the increase was a matter of mortality decline. In time, however, forces correlated with industrialization and urbanization brought on a fall in fertility, and so a slowing of the rate of increase.

It is the weakness of our understanding of the demographic transition elsewhere that we can not fully document, or for that matter precisely define, the immediate factors behind the fertility decline. Some (for example, delay of marriage) can of course be handled statistically. Certain of the pressures and conflicts of

the social structure can often be limned out quantitatively. But the attitudes, values, even fashions, of the individual decisions to control births are normally beyond such treatment for a large population. Like students of the problem in the West, Dr. Taeuber has had to confine herself to questions assessable in terms of formal data. But because she has used this data imaginatively she has given us one of the best pictures we have of the socio-economic transition of the past century, and even with these data she has done much to indicate which factors can, and which cannot, have been at work.

She shows, for example, that the gradual growth of fertility control before 1950 cannot be explained even to the extent it can in the West as a matter of the decline of nuclear familism and the growth of a corresponding individualism. Virtually all Japanese still get married and live in a consanguineously-related household where they are still subject to numerous traditional familistic pressures. Nor can the socio-economic transition be thought of as the growth of an insolently deracinated industrial and urban sector. Modern and traditional sectors are merely analytic poles of a smooth concrete continuum, held together by massive population interchanges, the dispersal of much industry in the countryside, and a healthy family industry in the city (and, one might add from observation, by a complete interpenetration of traditional and modern values). These demonstrations are stimulating for any explanation of the powerful role still played by traditionalism in Japan. Likewise of interest is Dr. Taeuber's thesis that its home, the rural sector, was in fact preserved by the process of modernization.

Apart from the gradual decline of the birth rate with the expansion of industrial and urban components (as in the West), Japan has had the unique experience of two periods of massive fertility control. For perhaps a century and a half immediately before modernization began, the population seems to have remained stagnant because of the widespread practices of late marriage and abortion among the upper classes, and of infanticide elsewhere. A century later a precipitous second decline carried the birth rate from its post-war bulge of 33 per 1,000 total population in 1949 to a little over 17 in 1957 and 1958—well below the pre-war trend line. The major factor was again widespread limitation, principally through the legal use of induced abortions.

Thus we are led to presume a distinct cultural tradition of limitation, probably dependent and appearing eccentrically, but still well known to all living Japanese as an acceptable way of

handling the problem of people. Does this make their experience incomparable with that of the West? In view of what Western birth rates have been doing recently, perhaps not, and in any event Japanese limitation was never fostered by a reigning ideology or by the political organs. Whether infrequent and gradually cumulative, or massive and abrupt (as in the West), control has been accomplished through the complexly-motivated decisions of individual families. But is not the Japanese experience then irrelevant to the future of those Asian nations now starting their demographic transition from vast population bases? Again, perhaps not. For one thing, Japanese limitation has not been anti-familistic. The extended family was never crucial, as it has ceased to be crucial elsewhere in large Asian populations today. The only firm demographic requirement of even old Japanese familism was for one son to carry on the line and rites—perhaps the major requirement also of Indian or pre-Communist Chinese familism. One might add that though no one pretends to understand the values and attitudes behind Japan's mass controls, conscious limitation must have a different meaning than in the West in a way of thought that is trans-dualistic, and in which a given life is seen to be to a high degree of only contingent value.

JOHN C. PELZEL

Harvard University

Population and World Politics. Edited by PHILIP M. HAUSER. Glencoe, Ill.: Free Press, 1958. 297 pp. \$6.00.

This volume is a symposium of twelve public lectures and papers given at the Thirtieth Institute of the Norman Wait Harris Memorial Foundation held at the University of Chicago in 1954. In the tradition of the Harris Foundation round tables, the conference included participants from a number of disciplines, in this case including sociologists, demographers, economists, anthropologists, political scientists, and historians as well as representatives of the clergy and of interested laymen. While specialists in population are in the majority among the contributors to this volume, they and other participants were encouraged to go beyond their fields of specialization. As the editor says, the contributors were urged to take "the risk of revealing themselves as citizens as well as scientists" (p. 20).

This was the third Harris Institute to give major consideration to population questions. The proceedings of the first, thirty years ago, were published in a volume entitled *Population* (1930) with major papers by Corrado Gini,

Shiroshi Nasu, O. E. Baker, and Robert R. Kuczynski. The proceedings of the second, *Food for the World* (edited by Theodore W. Schultz), appeared in 1946.

The present book reflects the admirable attempt of the conference to break down the walls of specialization. As one of the contributors (Joseph Spengler) points out, accurate specialization can produce quite as inaccurate history as unvalidated generalization.

The list of contributions is a distinguished one including an introduction by Philip Hauser; a section on "World Population and Resources" by Durand, Notestein, and Woytinsky; papers on "Population, Levels of Living, and Economic Development" by Kuznets, Hagen, Thomas, and Spengler; and a section on "Population Policy and Politics" by Davis, Lorimer, I. Taeuber, and Wright.

As a participant the reviewer regrets that it was apparently not possible to include summaries of the excellent discussion that followed many of the papers. These interdisciplinary and informal exchanges seemed to the reviewer to be a very valuable part of the conference.

The problems covered by the conference are indeed timely. It may be observed parenthetically that the United Nations estimates of world population have been raised 300 million since the conference was held. In the same period the United Nations estimates of current world population growth have been raised from about 30 million a year (1.2 per cent to about 45 million (1.5 per cent) per year.

The views of the contributors varied widely as to the nature and importance of population problems and their reference to the world political stage. Irene Taeuber begins her paper on "Population and Political Instabilities in Underdeveloped Areas" with the suggestion that "Population difficulties and political instabilities are so intermingled in most of the underdeveloped areas of the contemporary world that they constitute virtually insoluble problems" (p. 237).

By contrast, W. S. Woytinsky concludes his paper on "World Resources in Relation to Population" with the statement that "The race is not between population growth and natural resources, but between technology and politics, in the broad Aristotelian sense of that term" (p. 75).

In general the papers are well-written, technically excellent, and placed in good logical succession. It is nevertheless true that the title is somewhat of a misnomer. Only the last third of the book is devoted in a strict sense to the relation between population and politics. With the exception of Kingsley Davis's paper on

"Population and Power in the Free World," there is a conspicuous lack of discussion of subjects that would come immediately to the mind of a political geographer—population distribution and the location of national ecumenes, the problem of population concentration and vulnerability, the relation of urbanization to national strength, political aspects of international migration, problems of minorities and ethnic succession, the qualities of population as measured in national statistics. Put in another way it would perhaps be fair to say that the demographers and economists dominated the conference and that the representation and contribution of political scientists were not as effective as they might have been—this despite a good closing contribution on "Population and United States Foreign Policy" by Quincy Wright. In this he succinctly summarized the sense of the conference in his statement that "The principal population problem of the United States is the overpopulated countries" (p. 270).

The book is a valuable record of a stimulating and constructive meeting.

DUDLEY KIRK

The Population Council

The Politics of Despair. By HADLEY CANTRIL. New York: Basic Books, Inc., 1958. xv, 269 pp. \$5.00.

The perceptual world of the Communist voter in France and Italy is presented in detail in this book through numerous quotations from open-ended interviews. Life experiences, economic situation, and the interpretation of events by the Communist press contribute to the distinct and self-contained character of this perspective. Differences in reactions to the Hungarian revolution of 1956 are also interpreted in these terms.

The interviewing was done in 1955-57 for the Institute for International Social Research, Princeton. It included an experimental approach in which respondents were shown "newspaper articles" presenting various anti-Communist arguments, followed by questions to reveal attitude change. Arguments based on similarity of goals and ideals were more effective with French Communist voters than those based on American self-interest.

Cantril's stress in this study is on the psychological organization of perspectives, more than it is on factors such as party organization and tactics, mass media content, or structures of interpersonal communication. Following the transactional approach of *The "Why" of Man's Experience*, he emphasizes the subjectiveness and selectivity of the protest voter's view of the

political world. In his preface, he urges psychologists to study "real, full-blooded problems." Though this is a wholesome admonition, Cantril runs the risk of jettisoning scientific procedure in following it. For example, he organizes responses about the following propositions: (a) faith is intensely personal; (b) faith involves some participation in the flow of events, some action, some doing; (c) faith requires a sense of assurance that means can be followed or devised to bring about the experiencing of intended goals; (d) for faith to be enduring, a goal must serve as a step to other goals one can choose; (e) faith requires that a sense of self-constancy be maintained; (f) faith is made real only when hope is confirmed. Granted that these propositions can guide one's study of the protest voter's perspective, one nevertheless looks in vain for operational definitions that will make it possible to retest and refine them.

The net result of this approach is to leave the reader in doubt as to the intended audience of the book. As a broad-gauge treatment of leftist voting in France and Italy, it lacks the perspectives that sociology and political science might supply. As a journalistic treatment of "the protest voter as a human being," it falls short of what many European newspapermen could do. As an exhortation to social psychologists, perhaps it will provoke significant and rigorous studies related to the political problems of our time; it is to be hoped, too, that the Institute for International Social Research will itself pursue such studies. Among these studies might well be one of political opinion in the United States, entitled "The Politics of Complacency."

DUNCAN MACRAE, JR.

University of Chicago

Race: Individual and Collective Behavior. Edited by EDGAR T. THOMPSON and EVERETT C. HUGHES. Glencoe, Ill.: Free Press, 1958. x, 619 pp. \$7.50.

Here is another book of readings to join the company of Rose's *Race Prejudice and Discrimination*, Barron's *American Minorities*, and the old, but excellent, *When Peoples Meet* by Locke and Stern. This reviewer confesses that he has never found books of readings particularly satisfactory for his courses, and even less so now that sociological classics are becoming increasingly available in cheap, paperback editions. However, the rate at which they continue to be published must be an indication of their popularity, and teachers who find such volumes useful will indeed want to have a look at this one.

The collection consists of 139 separate items and includes poems, newspaper stories, fiction, judicial decisions and sketches, as well as articles from scholarly journals and excerpts from major books. The contributors are equally varied, and among them one finds Kipling, Toynbee, Schulberg, Spengler, Bercovici, H. S. Chamberlain, and William James. One of the most valuable features of the volume is the extensive and discriminating bibliography which covers 43 pages and contains nearly fifteen hundred references.

The editors give their point of view in these words: "This book [is] organized around the race problem. What is this problem? . . . Fundamentally it is the problem of understanding and dealing with an idea, the idea of race. . . . The idea of race is only one of many schismatic myths which have divided mankind into warring groups. . . . The idea . . . is beginning to decline before the facts of biology, of politics, and of human nature."

The various contributions are classified under eight general headings which reflect the editors' conception of the field of race relations and the framework within which they deal with it. Part I is entitled "The Need to Know Who We Are" and the readings illustrate a variety of topics: the difficulties individuals face in forming their own self concepts, the problems the courts have faced in making racial classifications, various popular classifications based upon language, religion, etc., and, finally, the anthropological and statistical conceptions. Titles of the remaining divisions are "Race and Region," "The Ecology of Race Relations," "The Idea of Race," "Race Conflict," "Status and Change," "Race and Human Nature," and "The Study and Control of Race Relations."

Teachers who focus their courses on the contemporary American situation will hardly find this volume suitable. Relatively few of the contributions bear upon the current economic, political, and social problems of minorities in the United States. Those, however, who approach the subject from the cross-cultural, comparative, world-wide point of view will find much in this volume to commend it. The book has the merit of giving historical depth and geographical breadth to the field of race and ethnic relations. This reviewer, who deplores the provincialism which characterizes so much of the teaching of sociology in American colleges and universities, recommends this volume for its objectivity, balance, and perspective.

BREWTON BERRY

The Ohio State University

Behavior and Evolution. Edited by ANNE ROE and GEORGE GAYLORD SIMPSON. New Haven: Yale University Press, 1958. viii, 557 pp. \$10.00.

This book is a product of two conferences attended by selected members of the American Psychological Association and the Society for the Study of Evolution. Twenty-two papers discussed at the conferences are here reproduced, representing a veritable galaxy of bright stars of zoology and comparative psychology: Simpson, Colbert, Romer, Beach, Pribram, Tinbergen, Harlow, Mayr, Washburn, Julian Huxley, and others. The objective is to explore possibilities for integrating the study of behavior into evolution, "to demonstrate that morphology, physiology, and behavior are aspects of organisms all inseparably involved in and explained by the universal fact of evolution. . . ." The essays vary from quite general to quite specific. An epilogue by Simpson valiantly attempts a synthesis.

Despite the brilliance, scholarship, and erudition represented, the total effect is disappointing. Many of the essays are inconclusive, some seem to be irrelevant to the question. Perhaps the reason is that evolution and behavior are too frequently thought of as separate *things*—one developmental and largely structural, and the other non-temporal and functional—which are to be somehow fitted together. Actually, in an evolutionary perspective, behavior is properly considered an *aspect* of the general evolutionary process. (The contrast will be illuminated by comparing the title of the book with the statement quoted above.) Evolution is not distinct from behavior, but is a process having structural (morphological) and functional (behavioral) aspects. Because the fossil record gives direct evidence only of structure, the functional aspect of evolution has been somewhat neglected—for which this book begins to atone. The central objective is properly the study of behavior *in* evolution, and the crucial problems would include: description of behavioral concomitants of structural development, integration of functional criteria in evolutionary classifications of life, the role of behavior relative to structure in effecting evolutionary change, the use of known evolutionary developments to explain why different organisms behave differently, and the functional aspects of such evolutionary results as adaptive radiation, dominance, progress, and the like. The most successful articles in the book are those specifically addressed to such questions, as that by Colbert, which includes a fascinating evolutionary explanation of why dogs differ from cats, by Romer on the functional concomitants of vertebrate evolution, by

Washburn and Avis, which convincingly relates morphological to behavioral changes in the development of man, and by Simpson, Mayr, and Pittendrigh.

There are discussions of cultural evolution by Julian Huxley and Margaret Mead. The selection of these authors for this topic is very curious, and perhaps unfortunate. Huxley is, of course, a brilliant biologist, but his experience in the professional study of cultural variation has been limited, to say the least, while Mead was trained in a school of anthropology (the Boasian) of which a principal tenet has been opposition to the study of cultural evolution. Huxley, in moving from biological to cultural evolution, from other organisms to man, seems to abandon a materialistic, naturalistic perspective in favor of an ideological, if not spiritual, one—like a movement from Thomas to Aldous. He sees cultural evolution as powered by "what I may, perhaps provocatively, call the group's current vision of destiny" (p. 439). Mead says nothing sensible about cultural evolution, and even makes the astounding assertion that her own social psychological studies are (cultural) evolutionary.

MARSHALL D. SAHLINS

University of Michigan

Sociology: The Study of Human Relations. By ARNOLD M. ROSE. New York: Alfred A. Knopf, 1956. xv, 589, xx pp. \$6.00.

At first blush, Professor Rose's introductory text seems not merely adequate, but impressive. In fact, two earlier reviewers praised it rather highly. After having used this book for nearly two years in our introductory course, I must register a minority report.

The book is verbose, somewhat disorganized, and surprisingly full of terminological and conceptual confusions. It lacks the intellectual rigor of Kingsley Davis's *Human Society*, the sophisticated sociological approach of Broom and Selznick's *Sociology*, and the style, charm, and precision of Bierstedt's *Social Order*.

The text's format is conventional but adequate. There are chapters on social control, the family, stratification, voluntary associations, social change, the urban community, etc. There is but very brief consideration of economic, political, and religious institutions. In the introduction, Professor Rose claims that "symbolic interaction" is the underlying theoretical point of view, and the book begins with a discussion of communication, role taking, and the development of the self. There is a rather good chapter on population, but as with the rest of the book,

there is too much descriptive detail and not enough theoretical perspicacity.

Extremely long-winded "examples" of dubious sociological value make for a flabbiness of style which contributes to a loss of interest on the part of the student reader. A more serious criticism than merely the stylistic one is the ambiguous, outmoded, or mistaken notions that Professor Rose has of certain key sociological concepts, for example, norms, personality, and institutions.

"Institutions in our culture," Rose says, "include the family, the church, the government, the schools, business enterprises, trade unions, public relief system, recreational facilities, voluntary associations of all types, libraries, museums, courts, jails and dozens of others." It is obvious that he has confounded institutions with groups, associations, and other phenomena.

In the chapter on personality, Rose perpetuates what to this reviewer is an unnecessary, even harmful fragmentation of personality into "biogenic personality," "psychogenic personality," and "sociogenic personality." Even if one were to accept this trichotomization, however, Rose does not work it through in an adequate manner. For example, he defines psychogenic personality "as those elements in the mental make-up which arise out of certain experiences not involving other persons. . . ." "It is generally held [by sociologists?] that the important psychogenic characteristics which become deep seated in the individual's personality are those which arise out of experiences occurring in the formative period of early infancy. . . ." What kinds of experiences are these that supposedly do not involve relations with other people; and what about childhood, adolescent, and adult experiences?

There are many nuggets of sociological insight, and excellent summaries of classical theories and studies scattered throughout, but the book as a whole leaves something to be desired.

ARNOLD LEVINE

University of Massachusetts

Cultural Anthropology: The Science of Custom. By FELIX M. KEESING. With illustrations by the author. New York: Rinehart & Co., Inc., 1958. xxv, 477 pp. \$6.00.

Anthropology belongs in a liberal arts curriculum because it opens to students new perspectives on humanity and human institutions. These orientations are not a part of every-day American culture nor are they anticipated to any great extent in a student's prior education.

Taking introductory anthropology can be an exciting and disturbing experience for students as they come to question many things that they and their fellow Americans usually take for granted and as they find, in doing so, a whole new world, much of it still uncharted, opening before them. An introductory course which fails to excite and disturb is not doing its educational job. When it appeared as a text twenty-five years ago, the clear prose of Linton's *Study of Man* had just this effect on students. It did not try to say too much, but concentrated on the development of a few key concepts and ideas.

Quite apart from this objective of significantly educating students, there is another aim often associated with introductory courses, that of giving the student an over-all view of the factual content, principle findings, and technical terminology of a particular discipline—the things that anyone claiming to be informed should know. Kessing's new text aims at this second objective.

Comprehensive coverage of the content of cultural anthropology is achieved through eighty-four different "problems," each introduced by asking a question to be answered in the ensuing discussion. Thus, "Problem 17. Man as Food Gatherer" starts with the question, "What is known of the origin and the early development of culture?" (p. 87). These "problems" are grouped into chapters in accordance with traditional classifications of anthropological subject matter. The comprehensive coverage and eclectic treatment allow no room for systematic development of the subjects discussed. Indeed there is an average of but five pages devoted to each "problem." The student is presented with a necessarily very brief sketch of what has been said by various authorities. Points of view are summarized in little outlines and there are countless definitions covering nearly every "technical" term that has ever been proposed in anthropology. The student is well briefed for the next quiz, but what he learns is open to serious question. Take the subject of religion for example (pp. 323-341). Three-sentence summaries of different theories about the origin of religion, a few paragraphs each on magic and myth, and one short paragraph on other forms and aspects of ritual, with a brief illustration (a short paragraph usually) from a specific society thrown in here and there—such procedures can give students no new insights at all.

Kessing frequently refers to the anthropological point of view in giving the answers to the questions that he raises, but to give answers is not the same as communicating the

point of view from which those answers reasonably follow. While such terms as "cultural" and "function" are frequently used, the important and, for the student, difficult concepts for which they stand receive cursory explication.

If we turn from the student to the teacher, especially the non-anthropologist who finds himself having to teach an introductory course, Kessing's text has several features which commend it. The bibliographies at the end of each chapter are with rare exceptions excellent. The coverage is extensive. The teacher can feel sure that for most "problems" all major theories are touched on. Common classroom questions are anticipated, being raised and answered in dialogue form. Some of these features also recommend the book to graduate students in anthropology for reference and review purposes. If it were about twice its present length, giving somewhat fuller treatment to the same topics, the book would be a very useful manual. For this is essentially what it is, a handbook of cultural anthropology.

WARD H. GOODENOUGH

University of Pennsylvania

Method in Social Anthropology. Edited by M. N. SRINIVAS. Selected Essays by A. R. RADCLIFFE-BROWN. Chicago: University of Chicago Press, 1958. xxi, 189 pp. \$3.75.

In his preface, Dr. Eggan comments that this is very probably the last occasion on which Radcliffe-Brown's published and unpublished papers will be collected into a volume. Several such collections have already appeared in the last six years.

The present volume is composed of two sections. The first includes five addresses or essays in which Radcliffe-Brown defined the scope of Social Anthropology and attempted to outline its methods. Four of these have been previously published and are already well-known to students. They include "The Methods of Ethnology and Social Anthropology" published in the *South African Journal of Science* in 1923, "The Present Position of Anthropological Studies" which was his Presidential Address to Section H of the British Association for the Advancement of Science in 1931, "Meaning and Scope of Social Anthropology" which appeared in *Nature* in 1944, and the 1951 Huxley Memorial Lecture, "The Comparative Method in Social Anthropology," published in the *Journal of the Royal Anthropological Institute* in 1952. The fifth essay is the abstract of a paper read before the fourth Panpacific Science Congress in 1929, and deals with much the same material as the other four. The second section of the volume is composed of the five chapters of the unfinished

introductory book on Social Anthropology which Radcliffe-Brown wrote between 1946 and 1950.

When a man has played a crucial role in the development of a discipline, as Radcliffe-Brown did in the development of Social Anthropology, it is good to have his work easily available in compact form. This is especially true since so much of what he wrote appeared in publications not ordinarily available to the student. For this reason, the present volume is a welcome and useful publication. On the other hand, the previously published essays included here are not his best work. The collection published in 1952 under the title *Structure and Function in Primitive Society* shows Radcliffe-Brown in his strength. There is little in the present volume which would give the newcomer to social science a clue as to why Radcliffe-Brown had the impact that he did. The first five essays repeat again and again the distinction between Ethnology and Social Anthropology and describe what Social Anthropology should do, but there is little indication of what was actually being done. The essays were important in their day, but their day is long past.

The latter portion of the volume is another matter. The first three chapters are concerned with a definition of the field and give a brief account of its development, beginning with precursors in the sixteenth and seventeenth century. These are useful though sketchy, and one wishes that Radcliffe-Brown had written at more length. The fourth chapter is a discussion of the concept of social structure, while the final chapter deals with the theory of social evolution. This last is highly important for an understanding of one strand of Radcliffe-Brown's thinking; for despite the frequent accusations that he was opposed to all use of historical speculation, he here declares himself as "one who has all his life accepted the hypothesis of social evolution as formulated by Spencer as a useful working hypothesis in the study of human society."

With the essays collected before one, it is very evident how early Radcliffe-Brown formulated his thinking about the scope and method of Social Anthropology, and how little change he made in the subsequent years. Professor Srinivas, who was one of his students in the later years at Oxford and who has edited this volume, provides a most valuable introduction which traces the connections between the various essays and also indicates some of the intellectual influences which helped to shape Radcliffe-Brown into the thinker that he was.

E. COLSON

Boston University

Nigeria: Background to Nationalism. By JAMES S. COLEMAN. Berkeley and Los Angeles: University of California Press, 1958. xiv, 510 pp. \$7.50.

We are faced today with much talk about colonial nationalism combined with a notable absence of authoritative information about it. The odysseys of journalists and the autobiographies of politicians, however inspired their writers, are no substitute for the careful and impartial studies of historians and social scientists. But such studies are usually too academic, involved, and pettifogging to appeal to any but extreme specialists. One can almost count on one hand popular books on contemporary West African development which are also informed and accurate. This is one of them.

The book is not, as Dr. Coleman maintains in his introduction, a study of the social and historical background of nationalism in Nigeria—this background is dismissed in the first 141 pages. What he has actually written is an introductory history of the rise and acceptance of Nigerian nationalism and of the establishment of its three regions as a self-governing federation. Although the work was planned with the terminal date of 1952, which coincided with the end of his fieldwork, subsequent events have developed his judgment and his appreciation of some specifically Nigerian developments, notably the "regionalisation of nationalism." He has very wisely, therefore, brought his book up to date by summarizing political developments up to 1957 when the struggle for Nigerian independence was settled. The result is that we have in a compass of 400 pages a survey of the whole of this crucial period of Nigerian political development. This is a very considerable achievement for a man who was able to spend less than two years in Nigeria and Great Britain. It is also a remarkable effort in compression.

Such compression has, naturally, its disadvantages. He is unable to present sufficient detail for the reader to draw his own conclusions and he is forced into expressions of opinion which one has to take on trust as well as into some generalizations which are too broad and which need qualification.

Still, it is not possible in an introductory work of this sort to indulge in detailed and qualified statements. Nor would it be feasible at this stage to do so; the available data are not sufficient and the rate of change is too rapid for one to distinguish clearly the significant from the insignificant. What is needed, and what Dr. Coleman has produced, is a general overall picture of the situation as he and as Nigerian politicians and British officials see it

at the moment. He has written a book that should be in the hands of all those people who intend to visit, to serve, or to study in this British Protectorate. It is one that should also be on the shelves of Nigerian political leaders and retired British proconsuls to remind them in the years to come of what they once said and believed they were doing.

G. I. JONES

Cambridge University, England

Durban: A Study in Racial Ecology, by LEO KUPER, HILSTAN WATTS, and RONALD DAVIES. With an introduction by ALAN PATON. London: Jonathan Cape, Ltd.; New York: Columbia University Press, 1958. 254 pp. \$3.75.

Urban sociology began, it has been asserted not wholly facetiously, because universities with departments of sociology existed in cities which offered handy workshops in which the professors and students could do research. The Union of South Africa has had both requirements for some decades. Urbanization began a good deal earlier and has gone further here than in other parts of Africa, and whereas other African countries are fortunate if they have one university, the Union has nine relatively old universities, several of which have departments of sociology. It is not surprising, therefore, that systematic research into urban conditions began here a good deal sooner than elsewhere on the continent. Recently, however, urban sociology has suffered in the Union itself—perhaps because of a certain exodus of intellectuals from Afrikanerdom nationalism and because of involvement with more immediate issues on the part of those who remained.

The fact that *Durban* has been published by Jonathan Cape with an introduction by Alan Paton (rather than as part of the Social Survey of Natal series), suggests a desire to reach a wider and more "popular" readership. This can be understood only in terms of the immediacy of the issue of the Group Areas Acts—which are here examined in intelligent and sociological fashion.

The first half of the book will be difficult for a non-sociologist, interested only in the political question, because the statistical and demographic analyses of census and other data are presented so extensively in tables, charts, and maps as to be wearying to the uninitiated. In this respect it is amusing to have a novelist introduce an established sociologist's book; the reverse would make more sense.

For the American sociologist, this study offers a competent analysis of a city of a particular type, and of a system of segregation that makes

the American South look simple by contrast. From either point of view I recommend the book highly. The shortcomings of the book are not those of the authors but of the system of analysis which is more that of Firey than that of the Lynds. Exegesis of census data can give understanding of social relationships, especially when done by someone who knows the community, but at some points it can only produce hypotheses. Then it would be helpful to follow up with pertinent interviewing in the relevant segments of the community. But one cannot hold against the authors, who have worked so intensively, for not having gone beyond the framework of their study.

DANIEL MCCALL

Boston University

Pilot Project, India: The Story of Rural Development at Etawah, Uttar Pradesh. By ALBERT MAYER and Associates. In collaboration with MCKIM MARRIOTT, and RICHARD L. PARK, with a foreword by PANDIT GOVIND BALLABH PANT. Berkeley & Los Angeles: University of California Press, 1958. xxiv, 367 pp. \$5.50.

Dirt, disease, malnutrition, overcrowding, eroded fields, meager crops, and a pervasive hopelessness—this stereotyped picture of India's villages is only too real. Yet past efforts to introduce changes have been curiously unsuccessful.

This significant book describes a pilot project in rural development which broke through the barriers of resistance and apathy. It began in 1948 in 64 villages in Etawah district, Uttar Pradesh. Within seven years, the villagers were transformed from passive recipients of help into active initiators and executors of plans for their own welfare.

Albert Mayer's story takes the form of connected extracts from contemporary documents, mostly his own reports and letters. We trace the gradual refinement of his plans, from early discussions with Nehru in 1945-6 to the inception of the project; we learn with him from the mistakes that were made; we share his cautious pleasure in the ultimate results. The autobiographical flavor of the book is no bad thing, because the qualities that Mayer displays—faith in people and in his purpose, inspirational leadership, infectious energy, capacity for self-criticism and for learning from experience, and of course, an intuitive grasp of the social processes involved—are probably essential ingredients of success in work of this kind.

Mayer's criterion of effective rural development is that it should survive and perpetuate itself: he speaks of "development within the villagers themselves" (pp. 235-6). It is there-

fore useless, if not impossible, to concentrate on developing "a single technical 'lobe'" (p. 175). Progress has to be made on a broad front.

To obtain the trust of the villagers, Mayer and his co-workers began by helping them to satisfy their most strongly felt needs—improved seed, soil conservation, better roads and housing, curative (rather than preventive) medicine. In the process—by substantially increasing crop yields, for example—the village acquired a sounder economic basis. The way was then paved for introducing improvements, such as preventive medicine and other public health measures, where the immediate gains were less obvious. At the same time, through constructive use of existing institutions, they began "to involve the villager not only on the mental plane and for economic reasons, but on the emotional plane as well" (p. 174).

Through this re-educational process, villages attained a new sense of unity and were enabled to work collectively for the common welfare in a way that caste and other forms of factionalism had previously made impossible. The *shramdan* ("labour-giving") movement, whereby the entire population of one or several villages worked on communal enterprises such as road-building, was a happy marriage of the Hindu ethic with current development needs, and it spread swiftly within and beyond the pilot project area.

Use of the "village level worker" was an important innovation. This versatile worker encourages and participates in the developmental activities of a small group of villages and whenever necessary commands the services of technical specialists covering a wider area. Collaborative relationships were fostered among all development workers, regardless of rank, and one of Mayer's major triumphs was to establish such a "democratic" unit within India's authoritarian administrative hierarchy.

Unfortunately, the Indian government is making a much too precipitate attempt to spread Mayer's work throughout the country, and in most areas results are bound to be superficial and ephemeral. In the pilot projects, however, there is every sign of permanence.

At the end of this fascinating account Mayer speculates soberly about possible effects of development on existing institutions, especially caste and the joint family. Will they be revitalized or "swept out . . . by extreme manifestations of a possibly anarchic over-individualization" (p. 337)? But nowhere does he mention the formidable annual increase in population, which threatens to nullify all efforts to raise the Indian standard of living.

E. J. MILLER

Tavistock Institute, London

Leadership and Power in the Chinese Community of Thailand. By G. WILLIAM SKINNER. Monographs of the Association for Asian Studies No. 3. Ithaca: Cornell University Press, 1958. xvii, 363 pp. \$6.50.

There is some question as to the justification of this book's title. In the narrow sense it is about leadership in the Chinese community in Thailand, but is it about power? Skinner's theoretical coordinates in dealing with power are derived from Harold Lasswell and Abraham Kaplan. Thus, "influence" is defined as "value position and potential," where a "value" is "a desired or goal event." The exercise of influence "consists in affecting the policies of others," and leadership is precisely this use of influence (p. 79). Power is defined as "participation in the making of sanctioned decisions." What is lacking in Skinner's book is precise detail on situations involving decision making. In short, we are brought up to the periphery of leadership and power arrangements but never passed beyond to a genuine understanding of the actual control processes and their significance. For this reason, the present reviewer finished the book with a strong feeling that the present alignment of leadership positions and personnel among the Thai Chinese could be swept away by portending events which cast only dim shadows on the pages of this volume. It is of interest that Skinner is aware of this difficulty and his comment which echoes our major reservation, is found in a footnote: "A more intensive study than has yet been made of Chinese culture in Thailand is necessary, however, before any definitive statements about the prevailing hierarchy of values can be made" (p. 83).

Despite the critical nature of the preceding remarks, there is no question in the reviewer's mind of Skinner's remarkable ability and analytical intelligence. The present work, like his former *Chinese Society in Thailand*, is a major contribution, not only to the rather tight little field of studies of the Overseas Chinese, but to the literature of social science. I am not competent to evaluate Professor Skinner's sampling techniques or his methods of establishing statistical reliability for the overwhelming quantity of data which he collected in 1952 and 1955. What emerges from this data, however, is a remarkable picture of the formal roles of leadership, their personnel, and the amazing network of relationships which connect the statuses of leadership. More than this, the juxtaposition of the results of two studies done three years apart and in two different external political climates (the official Thai policy vis-à-vis Communist China being the major variable) gives the analysis some remarkably dynamic qualities.

In conclusion it should be stated that the title of the work is likely to discourage sociologists who do not have specific interests in Southeast Asia, or in Chinese society, from working with it. This would be a most unfortunate fate for a fine contribution to the slim literature on the precise analysis of leadership and the composition of elites. The work, for example, makes explicit the character of minority group leaders as peripheral to the sub-societies which they lead, and adds to our understanding of this phenomenon in our own culture.

MORTON H. FRIED

Columbia University

Machine Age Maya: The Industrialization of a Guatemalan Community. By MANNING NASH. Glencoe, Ill.: Free Press; The University of Chicago, Research Center in Economic Development and Cultural Change, 1958. vi, 118 pp. \$5.00.

Nash neatly summarizes his study on the first page: "Today, in the highlands of Guatemala, a people still speaking Quiché, the women yet in costume, the world view of spirits and saints largely intact, have learned how to co-exist with a factory regime. . . . The study of Cantel serves two chief purposes. First, it tells of the way this particular community evolved mechanisms enabling it to adjust to a new mode of production with relatively little cultural loss or social disorganization. Second, Cantel's experience sheds light on the process of industrialization itself. . . ."

Cantel is a town of almost 2,000 people, surrounded by another 6,500 in rural settlements who belong to the same community. The textile mill was established in 1876. At first, only outsiders and marginal persons from Cantel accepted jobs. During the early years there was much unrest, for low wages, a transient work force, arbitrary authority by management, and suspicion and hostility on the part of a tight local community that resented invasion by outsiders, all combined to create a tense atmosphere. But accommodations were made: the management adjusted hours to the rhythm of local life, raised wages, hired Cantel people and allowed them to be trained and supervised by native foremen, accepted a union, and sought the co-operation of community leaders. In return, the townsmen found that they could gain steady work at good wages and not give up their traditional community life. By 1954, 500 men and 400 women, mostly natives of Cantel, were working in the factory. However, most office and skilled workers, and all the managing engineers, were outsiders.

The factory workers were people with in-

sufficient land to earn a living at the level set by custom as adequate. Factory wages were double those of agricultural day laborers, and provided a living equal to that of the richer farm owners. Factory people thus tended to consume a little more than the average farmer, but they consumed about the same things. Their family lives (based on the traditional nuclear family) were similar to those of farmers, except in the instances where a mill job was held by someone other than the male head of the nuclear family, which somewhat disturbed the usual focus of authority. Factory experience with the rationality of machines did not generalize and affect religion or cosmology.

Where, then, did industrial life change things? Friendship not linked to kinship was a new phenomenon that grew out of interaction in the mill. And the union, tied in with the left-wing program of the Arbenz government, gave the workers an instrument for local action and a tie to national affairs that was quite an innovation for Cantel.

Nash used the traditional informal techniques of social anthropology and used them well. However, his report suffers from the guesses he makes about the distribution of certain traits (such as the age structure of the work force and its family composition) which he should have checked by a systematic sample of factory employees. Furthermore, by concentrating on those who held semi-skilled jobs and eliminating consideration of skilled workers and office employees, he systematically underestimates the effects of the factory—even though most of the latter were not local people, they lived in Cantel and influenced its economy and its society. His theoretical conclusions hint at but do not thoroughly explore the special conditions that permitted Cantel to absorb the factory with so little change: the fact that one textile mill demanding low-level skills was not enough to create the widespread division of labor and urbanization that might lead to more drastic changes in the social order, or the fact that certain features of the pre-existing culture (a money economy, a nuclear family system) were conducive to industrial labor. His book is an excellent case study; its full implications await additional cases and more cogent theory.

JOSEPH A. KAHL

Washington University, St. Louis

Changing Images of America: A Study of Indian Students' Perception. By GEORGE V. COELHO. Foreword by GORDON W. ALLPORT. Glencoe, Ill.: Free Press, 1958. xxii, 145 pp. \$3.50.

Sixty Indian students were divided into four equal groups on the basis of the length of time

they had been in the United States. The students each wrote a brief essay on Indo-American relations which was coded for quantitative analysis. The same students were then interviewed to elicit imagined audiences for the communications. The procedure was repeated with fourteen students after an interval of six months or more to determine whether differences in the four groups were actually a function of time spent in the United States.

The study is said to be based upon reference group theory. Much of the material presented, however, concerns changes in perceptions with some analysis of the reorganization of cognitive fields. Reference group theory is used as an explanatory framework but there is little concrete analysis of the reference groups of the students investigated. Discussion, with few exceptions, refers only to host versus home country reference groups without further specification. There is no discussion of the influence of the foreign students or the Indian students in the host universities as possible reference groups. Nor, despite the heterogeneity of Indian culture, is there an indication of the status or regional origins of the subjects.

Despite the deficiencies implied by the foregoing, the study suggests that reference group theory supplies as satisfactory a framework for discussion as are those used in other recent foreign student studies. More importantly, the study confirms many findings which have emerged in other research upon foreign students, at the same time adding significant detail and illustration.

The existence of four phases in the foreign student's experience receives strong confirmation and the different ways in which the cognitive field and the perceptions of students change from one phase to the next is well illustrated. As in other studies, understanding or length of stay in the United States clearly is not associated with uncritical acceptance and "liking" of the United States. Two significant suggestions receive needed confirmation from Coelho's material. One is that, while perceptions tend to become differentiated and critical evaluation increases through time, this applies not only to the host country but to the home country. A second point of interest is that in the final phase, as the student becomes adapted to the host country there is actually a decline in perceptual differentiation, reaching its highest point among students who have decided to stay in the United States.

The present study will be useful to those interested in the problems of the foreign student. More importantly, it affirms not only the usefulness of further research on foreign students, for

the enlightenment it can contribute to problems of cross-cultural education, but the fruitfulness of foreign student studies for the cross-cultural testing of theoretical and methodological problems.

RALPH L. BEALS

University of California, Los Angeles

Person Perception and Interpersonal Behavior. Edited by RENATO TAGIURI and LUIGI PETRULLO. Stanford: Stanford University Press, 1958. xx, 390 pp. \$7.50.

Emotional Dynamics and Group Culture: Experimental Studies of Individual and Group Behavior. By DOROTHY STOCK and HERBERT A. THELEN. No. 2 of the Research Training Series. New York: New York University Press, for the National Training Laboratories, 1958. xviii, 296 pp. \$6.00.

Though jointly reviewed here, these volumes are quite different. They share two major elements: a commitment to the importance of social perception and a "progress report" character. Nearly all of the papers in the Tagiuri-Petrullo volume first appeared at a "Symposium on Person Perception" aimed at making progress toward a synthesis of studies on the perception of others. The Stock-Thelen book reports on progress in the research application of Bion's concepts of group process.

In keeping with the symposium's organizing function, the 23 papers of the Tagiuri-Petrullo volume are not so much research pieces as they are framework-seeking efforts. Many of the seekers are well known (chiefly in psychology): for example, Solomon Asch, Jerome Bruner, Lee Cronbach, and Theodore Newcomb. Given such a line-up (not to mention others who are deservedly getting to be known), the reviewer's cliché—that this volume may be read with much profit—can be taken for granted. For example, it would be a grave mistake for anyone to work quantitatively on (let us say) "insight" without taking into account Cronbach's analysis of the problems attending such work.

It would be a mistake, also, to miss Asch's brief essay, "The Metaphor: A Psychological Inquiry," or Criswell's discussion of "The Psychologist as Perceiver." Indeed, one might say that the major shortcoming of the field this volume represents is that it has not yet taken seriously enough the difficulties that these three papers discuss. The first difficulty—to which Cronbach's paper is partially addressed—is the haste in operationalizing measures without first clarifying concepts. The second difficulty, reviewed in Criswell's paper on the scientist's self-image, is the dedication to rigor at a cost

in understanding (a dedication that is exemplified in the apparent reluctance to engage the experimental subject in conversation; he has tasks, but he is rarely interviewed). And the third difficulty, symbolized by Asch's interest in our metaphorical use of "the language of naive physics" to describe human events (we speak of "hard decisions," or "straight thinkers" for example), is that most of these studies do not help much in "lessening the gap that has, too long, continued between psychology and the humanities" (p. 94).

Concretely, this means that these investigators too actively work at keeping the subject matter "out there"; and in the divorce between themselves and their subjects, the questions lose their human force: there are questions about identification, but not very much about the problems of personal identity; there are questions about the implicit personality theories that the subject's trait ratings reveal, but not very much about downright cruelty, vanity, or affection. It is possibly a very private "perception" on my part, but I do not think that the failure to deal directly with value-imbued social relations is conducive to the solution of the central problems of this field, the problems of empathy, insight, identification, projection, and the like.

The Stock-Thelen volume must be taken even more literally as a progress report; and—to put the conclusion first—the impressiveness of the progress depends upon what happens next. The authors are understandably convinced about their viewpoint and methods; but many of the studies reported here carry little substantive conviction. The majority of them were carried out on one or two groups of trainees in group dynamics at the National Training Laboratories in Bethel. The special quality of these groups, taken together with the impossibility of replication and control at vital points, means that the questions that are asked cannot really be answered; this applies also to the basic question: How useful are the Bion categories and the operations that implement his viewpoint?

The Bion viewpoint stresses a distinction between work and emotionality. Both the group's "basic assumption culture" (emotional states) and the individual's "valencies" (predispositions) can be described by way of four "modalities": fight, flight, pairing, and dependency. The research questions center around these concepts. For example: Can one predict participation in the group's emotional cultures through measures of individual valency? (Minimally so, apparently.) Do particular valency types display characteristic sociometric choice patterns? (Yes; but it is no simple matter of like-chooses-like.)

In this volume (as in the Tagiuri-Petrullo book, and the work in this field generally), emotional categories are stressed over work categories; norms are of less interest than percepts; process is primary as against content. Running throughout are certain fundamental assumptions about what people are like. Thus, "... we want members to be 'emotionally involved' in the task; but nobody gets emotionally involved in a task. What we get involved in is our needs for status, our fears of deprivation, our desires for success, our attachments to one another, our fear of emotion, our desires for a rational world" (p. 251). I wonder.

MELVIN SEEMAN

The Ohio State University

Human Motivation, Probability and Meaning.

By FRED T. SCHREIER. Glencoe, Ill.: Free Press, 1957. xxi, 256 pp. \$5.00.

The author's purposes are to relate and translate the conceptions of the various "schools" in sociology and psychology; to combine the "acceptable thoughts" from each; to carry out this synthesis while respecting the intentional character of actions and their objects as a condition of the task; and to search for empirical laws expressed in quantified measures of causal relations with the use of a precise metric whose mathematical structure corresponds to the theoretical properties of psychological events.

The author assumes as a doctrinal matter that motivation categories are categories of causation applied to psychological phenomena. As a rule of criticism and interpretation he assumes that any psychological phenomenon that the investigator can conceive as the cause of an action has the logical status of a motive of the action. For the purposes of research, psychological phenomena are examined for the logical structures of cause-effect relationships. Statistics are considered "the quantification of certain logical structures." This doctrinal point is also the particular contribution of the book. The principal logical structures considered by the author are relationships of necessary cause, sufficient cause, and side conditions.

Starting from Yule's formulas, the author develops many methods and measures combining tabular and correlation analysis "but going beyond tabular analysis by devising coefficients for the measurement of relationships among discreet and dichotomous variables." Extended attention is paid to a simplification of joint correlation which is achieved by using dichotomized attributes. The paradigm of the analysis consists of the four-fold table with fixed marginals. The rows are treated as the absence or presence of the cause, that is, the motivating condition;

the columns as the absence or presence of the effect. Frequencies are translated into percentages which are conceived as probabilities. From this paradigm and with the use of easily followed concepts and operations of set theory the author develops such measures as (1) the deviation of a cell from its random value as a measure of association between a motivating condition and its effect; (2) the gross and partial differences in the proportion of the effect between those exposed to the motivating condition and those not exposed; (3) the difference between the differences in the outcomes of the several motivating conditions. The last represents the author's concept of "joint structure." The concept and measure of joint structure is recommended by the author as a way of handling the important property of psychological phenomena where the organization of a set of elements is itself a problematical constituent feature of the event and requires measurement in its own right. The author's methods are well within the grasp of those with an elementary knowledge of set theory and statistics.

Space permits only a few of many major criticisms.

(1) Although the author is preoccupied throughout the book with the conception of the intentional structure of psychological events, which he refers to as "meaning," his discussions lack clarity and rigor except momentarily in his interesting discussion of attitudes and objects; for the most part the discussion is clouded by polemical interests. This is unfortunate because a review of the inadequate solutions to the problem of meaning in use in the various "schools" was barely started by Floyd Allport. The author by citing Husserl as a source of inspiration raises hopes that he will make the review from the standpoint of Husserlian phenomenology as the attitude and method of clarification. Nothing of the sort materializes.

(2) Max Weber is cast as a hero; Spencer, Parsons, Freud, Lewin, Asch, and Lazarsfeld are villains. With the exception of Lazarsfeld, the author's representations of the doctrines that he criticizes are shallow and inaccurate.

(3) The author asserts as an "undeniable situation" that the psychology of the other person rests on inferences from directly observable phenomena. This too is the result of his doctrinal commitments. But Asch, Heider, and Schutz (among others who have paid close attention to how the other person is known as an object) show that the relationship whereby the other person as an object is presented through his appearances is not at all a relationship of inference. An attempt to handle those

presentational relationships whereby the other is seen requires a care for the nature of sign relationships that goes beyond the few and poor evidences that the author gives of his grasp of and concern for this problem.

(4) Finally, the author correctly stresses that constitutive phenomenological researches on the evidential meanings of logical categories yield products that are of use to psychology as data. But nowhere does the author distinguish Husserl's research on the constitutive phenomenology of pure grammar and the constitutive phenomenology of the logical structures of objects within the rules of everyday usage. Nowhere does the author make reference to the logical structures of objects of common sense thinking, of myth, of ceremonial actions, of practical interests. Nor is there any mention of the problem of the grammar of symbolic representations of motives such as Kenneth Burke has so illuminatingly discussed.

The author's statistical methods are undoubtedly useful, but not as solutions to the problems of human motivation, probability, and meaning, which are barely touched.

HAROLD GARFINKEL

University of California, Los Angeles

Human Potentialities. By GARDNER MURPHY. New York: Basic Books, Inc., 1958. x, 340 pp. \$6.00.

The distant future appears in many forms to our volunteer prophets. There are extrapolated evolutionary visions of fantastic humans with bulbous heads and rudimentary legs, brave new worlds of mechanized super-tyranny, and Utopias of every degree of radiance and repulsiveness. There are also images of a world without people, from destruction by radiation, pollution, plague, victory of the insects, or exhaustion of resources by an explosive population growth.

Professor Murphy, out of the ripeness of years of research and study in psychology as well as a considerable amount of reading and reflection in science and philosophy, has labored carefully to give us a more optimistic outlook as it appears in his mind's eye. It is a view which any scholar can respect, disregarding any reservations in matters of detail.

Complete knowledge of man as an animal, argues the author, will not serve to give us a picture of the future. Our present biological nature cannot be presumed to be the stable reality to which evolving social organization must achieve a complete adaptation. Both the animal and the society are and will always be evolving in relation to each other. The emergence of new relations is always unpredictable

from the previous state of components, and so there is every reason to suppose that the future will hold at least as many dramatic surprises as has the past. Extrapolation from present knowledge of man and society, of trends of change, will not furnish a reliable view of the times ahead.

To let Murphy say it his way (p. 325): "Lest our thesis should seem to have become obscure, let us say flatly here that we often look for human nature in the wrong place; we merely look inside the living system. We are trying, as it were, to get the golden eggs by killing the goose; or indeed by studying the pedigree of the goose we have hoped to find specifically where those eggs came from. Actually, they are not in the goose; they are not even in the life history of the goose or in the life history of geese on this planet."

The "third human nature" (biological man and social organization being the first and second) is the "creative thrust of understanding" and it is in this sphere of individual creativeness that Murphy finds his theme and title, "Human Potentialities." He sees inspiring possibilities of development of creativeness in present man through removal of the various barriers, emotional and cultural, to maximum intellectual development of the individual. Additional mental improvement is seen from an increase in our understanding of the conditions of creativeness. Furthermore, in the interrelations with the forever new emergent conditions of man and society, unlimited development of powers is suggested.

Conceding that destruction of mankind is always possible, Murphy thus foresees as most likely a tomorrow full of surprises, but with new kinds of human nature, tending always to develop in creativeness, and always to get more control over man's own destiny. The treatment touches incidentally on the appropriate emotions to accompany these views, and suggests that we need not fear, nor should the morality of control and planning bother us. When control is possible, failure to plan has as many dangers as does planning.

The argument makes reference to bits of evidence, judgments of scholars, established scientific doctrines, but is far from a scouring review of research information. The material will therefore serve more as a source of inspiration to the general reader than as material for a social scientist. With enough luck the book might become a best seller among casual intellectuals, unless its optimistic character should render it ineligible.

ROBERT E. L. FARIS

University of Washington

Culture and Personality: A Study of Four Approaches. By S. KIRSON WEINBERG. Washington, D. C.: Public Affairs Press, 1958. iii, 58 pp. \$1.00, paper.

It is hard to define the need for, or the contribution of, this essay. As a survey of the literature on culture-personality problems it is not comprehensive, and it largely duplicates accounts available elsewhere. Nor does it systematically probe with critical sharpness any particular questions of the relationship of personality and culture but rather discusses, once over lightly, a vast range of problems. Weinberg sets out to "describe the way each discipline [anthropology, sociology, psychoanalysis and psychology] has analysed (1) society and culture, (2) personality and personality development in its cultural aspects, and (3) the psychological facets of group behavior" (p. iii). This is well-intentioned, but is only very roughly carried out. The organization of the paper by "disciplines" is, I think, unfortunate: it presupposes a unity of approach within each discipline which, except possibly for psychoanalysis, is far from existing in reality. Differences of conceptual approach *within* any one discipline have been as marked as differences *between* disciplines. In a concluding section attempting to evaluate "areas of difference and controversy" and "areas of convergence," I found nothing either new or enlightening, since statements are directed to extremely crudely-defined questions—for example, "effects of early life on later behavior"—and since no single theoretical perspective is presented to give coherence and a common point of reference.

Even more disturbing, the author repeatedly confuses *methodological* and *conceptual* problems, jumbling differences of technique, differences of subject matter, and differences of conceptual baggage. The relationship between technical procedures and types of conceptual emphasis is an important problem but because of the failure to keep separate the two elements of the relationship, it is never clearly developed here.

The condensed form of the essay also conduces to incomprehensibility, and distortion.

Though omissions are unavoidable in such a work—there is the usual disclaimer of comprehensiveness—two of them are glaring, in that they are so intensely relevant to the rest of the discussion: Weston LaBarre's *The Human Animal*, and Merton's "Social Structure and Anomie."

ROBERT ENDLEMAN

State University College, Long Island

Personality: An Interdisciplinary Approach. By LOUIS P. THORPE and ALLEN M. SCHMULLER. Princeton, N. J.: Van Nostrand Co., 1958. v, 368 pp. \$5.50.

This textbook by two educational psychologists attempts to present a view of personality that is a "patterned eclecticism" integrating genetics, psychology, sociology, education, anthropology, and psychoanalysis. The main body of the text has three sections: Foundations of Personality—Biological Basis, Motivation, Affective Factors; Determinants of Personality—Environment, Cultural Determinants, The Home, Education; and Approaches to Personality—Psychoanalysis, Organization of Personality, Typing of Personality, Evaluation of Personality. The claim of "integration" is to be taken lightly. No single conceptual scheme orders the work. Rather we get a smorgasbord of everything from genes to dreams.

Of concern to sociologists is the section on Determinants. Considering that the authors are psychologists, they devote a sizable portion to work done by sociologists, social psychologists, and cultural anthropologists. They refer to studies of class and status (not well distinguished) as related to personality, make allusions to conformity, and some reference to socialization. Chapter 7, "The Role of the Home in Personality Adjustment," tries to deal with the impact of the family on personality in something vaguely like development sequence. Characteristically, it suffers from lack of any clear theoretical focus, either psychological or sociological, let alone articulation of two such theories. No conception of kinship as a variable social structure appears. Sociological references here are mainly to Sewell's and Orlansky's arguments against the importance of infant training. Other preoccupations of the chapter are "The Effect of Broken Homes," "Inadequate Sex Education," and "Inadequate Parent-Child Relationships." Similarly, Chapter 8, on Education (that is, formal schools in modern America), lacks any structural context for the analysis of schools and fails to relate schools to socialization. It stresses that schools should teach social studies to combat prejudice, teach reading for its psychotherapeutic value, and do other such nice things.

There is no reference to recent literature relating personality and social structure via some version of action theory (Parsons, Merton, Bales, Shils, Inkeles, Cohen, Aberle, etc.). Though conformity and the super-ego are referred to, the authors show no awareness of studies of internalization of norms. Reference to anthropological work is limited to more popular

works of Mead, and brief allusions to a few others, the treatment being superficial.

If a sociologist teaching a course on personality and society insists on using a textbook, we can tell him that this one (aside from its greater psychological emphasis) may suit him no worse than most others. It shares with them a grab-bag eclecticism that befogs difficult theoretical problems, a confusion of purposes (shuttling giddily among scientific, practical, and normative intents), a corresponding confusion in language and style, and an oversimplification and vulgarization of profound and thorny ideas about the human condition.

ROBERT ENDLEMAN

State University College, Long Island

Current Concepts of Positive Mental Health.

By MARIE JAHODA. Joint Commission on Mental Illness and Health, Monograph Series No. 1. New York: Basic Books, Inc., 1958. xvii, 136 pp. \$2.75.

Economics of Mental Health. By RASHI FEIN.

Joint Commission on Mental Illness and Health, Monograph Series No. 2. New York: Basic Books, Inc., 1958. xx, 164 pp. \$3.00.

It would be hard to overestimate the importance of the Joint Commission and its monograph series—of which these volumes are the first harvest—for the future course of mental health and illness in the United States. Authorized by Congress in the Mental Health Study Act of 1955, the Commission's findings may very well become a basis for federal and state action in research, prevention, and care. If the splendid, incisive accounts of Marie Jahoda and Rashi Fein are auguries of the Commission's total effort, the field of health and the professions of social science and medicine will owe a heavy debt to the director, Jack R. Ewalt, and his staff.

Marie Jahoda brings to her immensely difficult task fine qualifications in psychology and sociology, theory and research; she does a selective, sensitive, and intellectually economical job of presenting the core knowledge we have about positive mental health. Operating in an area of the behavioral sciences which is at once frustratingly vague and extremely salient to the study of man, she performs a masterly dissection, cutting away underbrush and preserving live conceptual oaks. From this emerge a creative synthesis of models of health and suggestions for empirical research. The book is an admirably balanced critique: philosophically sophisticated and aesthetically alert, but at the same time hard-headed and flavored with the modesty of mature thought.

Roving among the many efforts to define mental health (as embodied in a person who is "integrated," "mature," "real," "self-actualizing," "genital," "adjusted," and so on), Miss Jahoda concludes that the criteria of positive health cluster into a few central categories. These are: *attitudes toward the self; growth, development, or self-actualization; integration; autonomy; perception of reality; environmental mastery*. One can readily see that these clusters tend to stress some kind of general cognitive and emotional competence, the capacity for balancing the heart's desire with the way of the world. She proceeds to refine these categories and to elaborate certain techniques for assessing their degree of fulfillment in a given personality. The latter section sketches a variety of clues for fresh and exciting research.

There are several particularly notable elements in Miss Jahoda's orientation. She perceives the possibility that there are mental *healths*, rather than a unitary model of grace, and that these may be alike in "goodness" while different in their weightings of the various criteria. She deals sensibly with questions of value, viewing mental health as *one* value among others and refusing to tolerate the notion of health as a panacea. This volume sustains a rightly complicated approach to life, abjuring hydro-infanticide in favor of judicious estimates of assets and deficits, as in taking the disturbed artist (for example, William Blake) as valuable and healthy in work even if unhealthy in others of life's arenas.

To the sociologist, there is some natural disappointment at the failure to come fully to grips with the environmental mold which nurtures or thwarts individual achievement of health. But then, it is extremely doubtful that sociologists can do any better with this than Miss Jahoda does at present—and she at least proposes some intriguing leads.

Finally, and I think as a very welcome corrective to psychological ideals of health, the psychiatrist Walter Barton argues cogently in an epilogue for the utility of the clinician's traditional view of health as "the absence of illness."

Dr. Rashi Fein, an economist, complements the portrait of health desiderata by a clear, bold description of the negative side of the coin, the cost of mental illness to the nation. As in the first volume, the emphasis here is upon a brief but searching summary; the aim is not an exhaustive survey but a compact presentation of the most important economic questions posed by the incidence and prevalence of mental illness in the American population. While Dr. Fein sticks closely to the exercise of what

Kenneth Boulding calls "the skills of the economist," he does not fail to see economic forces in a wider social and philosophic focus. For instance, he states trenchantly the relationship of health expenditures to our value choices as well as to the sheer availability of our economic resources.

By a rather conservative reckoning, mental illness is shown to cost the United States about three billion dollars per year. Dr. Fein arrives at this figure by a compilation of direct and indirect costs; his method of deriving indirect cost is ingenious but cautious, being based primarily on computation of lost earning power by the ill. The totals merit special confidence because they deliberately omit any attempt to add in those less tangible indirect losses which must be substantial, for example, the drain on demoralized families in the absence of a vital member, the gap of creativity and productivity in professions and organizations when a key man is disabled, the alternative uses of medical personnel now engaged in treatment, and so forth.

Among the virtues of this account is the presentation of a method to enable an estimate of direct costs in relation to the patient's age, diagnosis, and length of hospital stay. This predictive formula should prove valuable to health planners.

All in all, Dr. Fein has given the advocates of intensive efforts to improve mental health a solid economic flooring and justification. He affords a calculated rationale for major expenditures on research, prevention, and therapy.

ROBERT N. WILSON

Harvard University

Symposium on Preventive and Social Psychiatry. By the WALTER REED ARMY INSTITUTE OF RESEARCH. Sponsored jointly by the Institute, Walter Reed Army Medical Center, and the National Research Council. Washington: U. S. Government Printing Office, 1957. xi, 529 pp. \$2.00, paper.

In his preface to this rich compendium, David Rioch identifies the central theme of an exceedingly diverse collection of papers as follows: "Whereas the early developments of modern psychiatry were concerned with the personal history and early life experiences of patients, the emerging theories lay emphasis upon current social roles and social environmental contingencies."

The latter orientation is so marked that this symposium might well be more immediately appealing to the sociologist than to the classically-trained psychiatrist with his traditional absorption in intrapsychic events. In any case,

both the organization of the book and the high quality of the separate contributions afford a stimulating overview of the burgeoning field of social psychiatry. Many of the authors' observations are dramatically fresh and challenging, a fact which may not be unrelated to the wealth of field and clinical experience they represent. These psychiatrists and social scientists are not talking *about* something so much as searching for comprehension of experience they have *lived in and with*, and their words have the tang of reality.

Although the expressed aim of the symposium was to contribute to the host's needs, and it is therefore laced through with considerations of direct pertinence to military psychiatry, it is by no means parochial in scope. The major sections range from theoretical foundations (communications and influence in group structure, ecology and epidemiology of illness) to organizational settings (industrial psychiatry, leadership) to community frameworks (social psychiatry in the community, the therapeutic milieu in the hospital). There is a continuing and salutary tension, in the papers and the discussions which follow them, between the immediate needs of the therapist or administrator and the no less urgent needs of the scientist. This tension reflects one of the chief virtues of the field of social psychiatry in that here experiment and practice, theory and action, are often intimately related in that constant shuttling between exigent application and ultimate comprehension which characterizes social life itself.

Psychiatric experience in military life, whether garrison or combat, has in the past decade helped to engender a revolutionary shift in orientation toward the patient and his interpersonal environment. The psychiatric casualty, who had in World Wars I and II been typically invalidated out of his unit and treated as a self-contained "case," has in recent years been returned to duty as soon as possible, kept with his unit, and treated as a momentarily shaken adult who is capable of adapting to his human environment and to a reality situation of stress. A variety of "commonsense" psychiatry has begun to play down the unique exfoliation of symptoms and individual trauma, and to emphasize the patient's positive capacities for maintaining himself as a functioning part of a social organization. Military practices have been complemented by the increasingly sociological stance of psychiatrists in industry, community, and mental hospital. In one sense, social psychiatry takes quite literally Sullivan's dictum that individual disorder has its locus in disturbed interpersonal relations, and goes the

further step of postulating that recovery also must occur in the interpersonal universe.

Successful performance as a group member may be dangerous if used as sole criterion of mental health; much obviously depends on the nature and permanence of the group selected as referent. Goffman in a brilliant discussion of "total" institutions limns the encapsulating properties of organizations such as the army or the hospital. The eternal dialectic of organizational goals and individual needs continues to plague the social psychiatrist as it has long riven the democratic philosopher. There are, however, several indications in this volume that the dialectic may be mentally healthy in import. In addition, there are a few efforts, notably those of Redl, Stanton, and Hamburg, to define more precisely the properties of psychologically benign environments and the details of linkage between social organization and individual life process.

ROBERT N. WILSON

Harvard University

Personality Patterns of Psychiatrists. By ROBERT R. HOLT and LESTER LUBORSKY. Volume I, *A Study of Methods for Selecting Residents*, xiv, 386 pp., \$7.50. Volume II, *Supplementary and Supporting Data*, xiv, 400 pp., \$4.00, paper. Menninger Clinic Monograph Series, No. 13. With the collaboration of WILLIAM R. MORROW, DAVID RAPAPORT, and SYBILLE K. ESCALONA. New York: Basic Books, Inc., 1958.

While not intended as a definitive study of what the personality qualities of psychiatrists are and ought to be, or of how these qualities are related to the processes by which physicians are recruited, selected and trained for the role of psychiatrist, this two-volume work nevertheless represents the first large-scale, systematic research project which bears upon these problems. Its aim is to "tell the story of a ten-year effort to learn how residents were being selected for the Menninger School of Psychiatry and how the job could be done better." Holt and Luborsky (who are both clinical psychologists) acknowledge the important role that social and cultural factors play in the selection, training, and functioning of psychiatrists, and say that "with the aid of expert consultants" they tried to "take into account . . . the milieu of the School . . . the place of psychiatry in the occupational structure and other aspects of the social system." Methodologically speaking, the authors have taken what they term a "sophisticated" clinical approach, relying heavily on data from projective techniques and clinical interviews. In

addition, other "so-called 'objective' tests" and professional credentials (grades, letters of recommendation, etc.) were employed.

Volume I of this study is divided into four parts. Part I introduces the problem, the subjects, and the setting in which they were trained and the research carried out. Part II gives a relatively non-technical account of the research, the two experimental designs followed, the methods used, and the findings. Part III is devoted to the personality of the psychiatrist; and Part IV presents the authors' conclusions and recommendations for the selection of psychiatric residents and psychoanalytic candidates. Volume II, which "contains detailed accounts of the research methods and many tables of quantitative findings," is arranged as a series of appendices to the chapters in Volume I and is intended to be read in conjunction with these chapters.

The primary value of this meticulous study lies in its systematic experimentation with various actuarial and clinical psychological tests and procedures in a rigorous effort to reach scientifically well-founded judgments about the merits and deficiencies of these methods for assessing and choosing physicians who apply for psychiatric training. In this regard the findings of the study are too specialized to be sociologically pertinent. However, there are a number of secondary ways in which this study ought to be useful, enlightening, or enriching to sociologists. Chapter VII, a thorough critical survey of the long line of vocational selection studies that deal with "fitting the right man to the job," and Chapter XV (and the appendix to it in Volume II), a summary of published and unpublished expert opinion on personality requisites for psychiatry, have value for sociologists interested in the interrelationship between personality and social roles, particularly occupational roles of a professional type. Because the authors have been so honest and detailed in telling the story of their study, these two volumes also represent the sort of richly documented case report of the actual processes of research that will have significance for persons interested in the sociology of science and the development of methodology.

RENÉE C. FOX

Barnard College, Columbia University

Psychological Stress: Psychoanalytic and Behavioral Studies of Surgical Patients. By IRVING L. JANIS. New York: John Wiley & Sons, Inc., 1958. London: Chapman & Hall, Ltd. xiv, 439 pp. \$6.95.

The author of this book is a Yale psychologist, well known in behavioral science for a

previous book, *Air War and Emotional Stress*, and for his experiments in communication and attitude change. In the present volume, he is engaged primarily in a search for "general laws of stress behavior" (p. 8). It is plausible, he believes, that systematic study of reactions to impending surgery, as well as of post-operative behavior, may yield generalizations that are applicable to a wide range of "physical dangers" (for example, air raids, disasters, concentration camp tortures, etc.) and perhaps even to such non-physically dangerous stresses as loss of status, or bereavement. For his study of psychological stress related to surgery he has used psychoanalytic case materials, interview data, and questionnaire responses.

Nearly half of the book is devoted to the psychoanalytic patient, the analysis of his pre- and post-operative behavior in analytic sessions, and the psychodynamic propositions formulated by the author. After examining all of his case notes, and taking into account findings of other researchers on stress, the author formulated fourteen basic hypotheses (and a number of supplementary propositions) aimed at specifying the mediating (and usually unconscious) processes that were at work in the patient's "typical" stress reactions—"typical" not to him alone, but to most persons in modern society, according to the author.

One example of the hypotheses will suffice:

Hypothesis 1A. Exposure to a threat of body damage (as in the case of an impending surgical operation) tends to sensitize the individual to unacceptable hostile and destructive tendencies in his own aggressive behavior, so that even relatively minor aggressive actions, which are normally tolerated without affective involvement, are consciously or unconsciously felt to be violations of inner superego standards (p. 54).

One central theme, explicitly stated, runs through all the hypotheses, namely, "that stress experiences tend to rouse apparently outgrown modes of response to childhood dangers" (p. 53, cf. p. 196).

The rest of the book is devoted to an analysis of 22 case studies of surgical patients, whom the author interviewed both before and after their operations, and to an analysis of the responses of 149 male college students to a questionnaire designed to elicit comparable information concerning their fairly recent surgical experiences. Both sets of data confirm the hypothesis that patients with a moderate amount of "anticipatory fear" (the antecedent, pre-operative variable) make the most adequate post-operative adjustments. On the other hand, high and low "anticipatory fear" are related, respectively, to concern with body damage and to externalized anger (consequent, "reaction variables"). Attention is also given here to the

problem of "psychological preparation" of the patient, and the concept of "the work of worrying" is introduced to help explain the results.

Such a brief description of the content of this book cannot begin to do justice to its intricate patterns, the subtleties of analysis, and the richness of theoretical formulations to be found here. In Chapter 2, and the Notes thereto, Janis very thoughtfully analyzes the limitations and research potentials of psychoanalytic case histories, one of the best treatments this reviewer has seen. His discussions of "unrepression" (Chapter 16) and the "work of worrying" (Chapter 25) are provocative and will surely receive wide attention from psychologists and psychiatrists. Janis deals explicitly with the limitations of his data, yet gets more out of them than many other researchers who have a basically better research design. In short, the book is a solid achievement, a complex study of phenomena of great importance.

This is not to say, however, that all parts of it merit equal acclaim, or that all readers will agree as to its merits or demerits. Indeed, many readers will find it overly long and unnecessarily repetitious. To those who are less than enchanted with psychoanalysis the book may be dull and boring and they may even wonder, for example in Chapter 6, how much of the patient's problems the analyst *revealed* and how much he *created*. Then, in his search for generalizations, Janis frequently resorts to such phrases as "the average person" and "the typical response." Sociologists might well raise questions about the adequacy of samples and the universe referred to (sometimes it seems to be all human beings; at other times persons in "modern society"). In a similar vein, the author frequently refers to "exaggerated" or "unreal" fears and perceptions, but the standards for measuring "reality" or "exaggeration" are by no means clear. Sociologists might claim that a variety of cultural assumptions have been smuggled into the analysis (unconsciously?). Again, while passing references are made to social and cultural variables, sociologists are likely to miss any protracted analysis of them. Indeed, it would be interesting to compare Janis' type of analysis with that of a medical sociologist such as Kutner, for both deal with the same kind of problem.

This book, then, is a notable attempt to deal systematically with the complex phenomena of stress, primarily from a psychoanalytic point of view. Despite some shortcomings from a sociological orientation, it will be required reading for all behavioral scientists concerned with the sources and consequences of stress behavior.

EDMUND H. VOLKART

Stanford University

Medical Sociology: Theory, Scope and Method.
By NORMAN G. HAWKINS. Springfield, Ill.: Charles C. Thomas, 1958. xx, 290 pp. \$6.75.

The title and format of this volume suggest that it might serve as a textbook for those who wish to gain a comprehensive view of that rapidly growing field identified by some as "medical sociology" and by others as "the sociology of medicine." The reader should therefore be warned in advance that Hawkins presents only a highly subjective *selection* of relevant topics, and that many medical sociologists would neither recognize nor claim the picture of the field that is thus provided.

In the author's words, the "... book will prove to be worthless to browsers, unintelligible to social workers, a disappointment for searchers after 'sophisticated technics,' and a glimpse into new worlds for the sociologist, physician, biostatistician, social psychologist, or anthropologist who has an interest in questions of health and disease" (p. xii). The glimpses offered to the latter elect persons are contained in six chapters which respectively concern: the history and definition of medical sociology; an interdisciplinary "matrix" theory of health, stress, and disease; aging and certain chronic diseases interpreted in terms of the theory described; the role and status of the medical sociologist; social factors in the demand for medical care and the organization of medical services; and statistical methods for analyzing data. These several subjects are not evenly treated, either in terms of specificity of problems discussed or in terms of references cited. (The seminal work of Parsons on the sick role is not mentioned, for example, and Oswald Hall's studies of physicians are similarly ignored.) Most heavily documented and perhaps most provocatively original is the author's analysis of chronic disease as related to stress.

The discursive style that characterizes the body of the book is carried into the glossary, where definitions of certain sociological and medical terms are presented. Some of these definitions are sufficiently vague and misleading as to constitute more of a hindrance than a help in understanding either medicine or sociology. For example, "benign" is described as "Not malignant (q.v.); uneventful; not symptomatic of need for medical intervention" (p. 218); yet as all physicians know, a benign tumor frequently indicates the need for medical intervention. Likewise most sociologists would find inadequate the conception of "profession" as "An institutionalized occupational category (*see* Institution). Professions are characteristic of advanced societies, i.e., those in which specialization and formal relations are dominant and

laws tend to replace mores and folkways as the framework of the culture" (p. 227).

At present this is the only book on medical sociology that attempts to define the scope of the field. The author's motivation is praiseworthy but the result is disappointing. Thus it may be hoped that the volume will be quickly succeeded by another more broadly and carefully contrived.

MARY E. W. GOSS

*The New York Hospital,
Cornell Medical Center*

A History of Public Health. GEORGE ROSEN.
New York: MD Publications Inc., 1958. 552
pp. \$5.75.

This is the first of a new series of monographs on medical history. This book traces the course of group action aimed at solving health problems from the earlier civilizations through to the present day. The author, who is presently Professor of Public Health Education at Columbia University and Editor of the American Journal of Public Health, has fused his own education, experience, and research in medicine, public health, sociology, and history into an interesting and perceptive book.

The history of public health is closely interwoven with the development of society itself, with its growth, its religions, its adaptation to scientific and technological advances, its forms of organization, social, political, and economic. Those interrelationships are clearly delineated in chapters whose titles themselves underscore these processes: for example, "Mercantilism, Absolutism and the Health of the People," and "Industrialism and the Sanitary Movement."

The book covers so vast an area of the development of human society and so great a span of time that its attention must be focused primarily on the highlights of public health history. This is, therefore, essentially a story of forward progress from the Greco-Roman world to the present time. The material relates chiefly to those countries in which progress has taken place. Thus, the emphasis is upon the last few centuries and is centered in the United States, Great Britain, France, and Germany. For these same reasons, the discussion is almost exclusively devoted to the forces and persons responsible for the crystallization and solution of problems. The scope of the volume itself did not permit the author to discuss such additional pertinent aspects as the social forces which obstructed public health progress.

The components of public health which are discussed include, *inter alia*, sanitation, maternal and child health, occupational health, medical

care, public health nursing, and the development of international health activities. There is an especially full discussion of the Bacteriological Era and its quick aftermath in the shape of specific program development.

The section on the organization of and payment for medical services, though necessarily compressed, presents a well-rounded insightful review of these, essentially social, developments in Europe and the United States.

While this book is especially pertinent to professional people in medicine, public health, sociology, and public administration, it would also be of interest and use to community leaders who have leadership responsibilities for health and welfare programs.

CECIL G. SHEPS

Harvard Medical School

Studies of the Family. Volume II. Edited by NELS ANDERSON. Séminaire 1955 du Séminaire International de Recherche sur la Famille et du l'Institut UNESCO des Sciences Sociales. Göttingen: Vandenhoeck & Ruprecht, 1957. xi, 337 pp. 24, —DM, paper.

This collection was prepared by participants in the 1955 International Seminar on Family Research, chaired by Professor Burgess. The theme of this meeting was "The Family and the Development of the Child."

Many of the papers clearly have this theme as a central focus. In general, the most interesting and detailed research reports are those which are most closely related to studies of child development and of the social structure of the family. This group includes the following: Burgess' progress report on a study of the effects of the marital relationship upon the development of the children (the study utilizes the families of 250 of the couples from his original marriage adjustment study); D'espallier's summary of the findings of four studies done in Belgium between 1944 and 1955 on the "only child;" Grønseth and Tiller's report on the effects of "father absence" in Norwegian sailors' families; Rijkssen's paper on the consequences in Amsterdam of the "incomplete family;" Kälvesten's presentation of a very ambitious study of "social adjustment and family relations of school boys in Stockholm;" M. J. and P. C. De Lauwe's report on the effects of working mothers on the parent-child relationship in Paris; and Haider's description of a study in Vienna of the relationship between parental behavior and three types of "socially conspicuous children: whipping-boys, group-clowns, and neurotic delinquents." Closely related are Baumert's paper on the changing role of the child in the German family and Himmelweit's report of some differences between

social classes in parent-child relations in London.

In addition, there are a report on the program of the "Ecole des Parents et des Educateurs" in Paris (Isambert), a general paper on the psychology of parent-child relations (Mauco), a discussion of social change and changing family relations in Finland (Nieminen), a short essay on the changing self-concept of farmers' wives in North and East Groningen (Saal), and what is largely a proposal to study the effects of rehousing on the family and the child in two Bristol housing projects (Spencer).

In his preface the editor states: "Seminar participants meet informally with their notes, their project plans, perhaps their unfinished reports. They go into matters of methodology, the analysis of materials and other phases of research." If so, this volume gives the reader an idea of what these sessions are like, for the papers touch variously on all these matters and range in completeness from pleas for research (Nieminen), to descriptions of the beginnings of yet vaguely defined researches (Spencer), to advanced progress reports (Burgess, Grønseth and Tiller, Kälvesten), to summaries of research already published in detail elsewhere (D'espallier). Prospective readers should be warned that the completed research reports are very few, but almost all contain interesting information.

In spite of the great variation in the quality and significance of the several pieces, this work has several important features which apply to the collection as a whole. First, it indicates that in family research in the West a number of common orientations are emerging, particularly role-theory and psychoanalytic theory (though Grønseth and Tiller modify this last in terms of Reich's "sex-economy" theory). Second, it indicates that similar research approaches to related problems are producing increasingly comparable data in the various nations. This was especially apparent in the study of social class differences in London (which had the added feature of an American sociologist on a Fulbright working with the study group at the London School) but it was noteworthy throughout. Perhaps most importantly, there is much evidence here that some of our European colleagues are undertaking unbelievably ambitious and significant researches with very limited resources.

The major drawbacks of this book are on the technical side. Although the four papers in French (De Lauwe, D'espallier, Isambert, and Mauco) and the two in German (Baumert, Haider) are followed by abstracts in English, in some instances the resumes are so limited in detail as to be of little use. This greatly restricts the use of this volume by most American undergraduates, and from this standpoint it is espe-

cially regretted that Baumert's article is not available in English. Furthermore, the articles are simply arranged alphabetically by author's last name. They are not even ordered in terms of their relevance—or irrelevance in some cases—to the central theme. This, coupled with the considerable incompleteness of many of the papers, leaves the reader at the end haunted by one overpowering question: Why were these materials published at this time in this form at all?

HARRY V. BALL

Pomona College

Mate-Selection: A Study of Complementary Needs. By ROBERT F. WINCH. New York: Harper & Bros., 1958. xix, 349 pp. \$5.00.

Earlier papers reported that Winch's sample of 25 couples had been chosen from married undergraduates at Northwestern University. Previously, such a sample had seemed to me to constitute almost an irreparable defect, but I had missed the author's logic. His couples were articulate, homogenous middle-class persons who had chosen their mates with relative freedom from parental intervention and with maximal dating opportunities. If the seeking of a mate within the field of eligibles so as to maximize need gratification (that is, complementarity) exists, this sample should manifest it!

The earlier finding that like marries like is quickly disposed of as a social mechanism for defining the field of eligibles. Complementarity can be of two types: the first involves high and low levels of the same need and the second, interdependent needs (like abasement and autonomy). If the author had been in the Murdock tradition, he could have argued that complementarity leads to the presence in each family of a broad range of adaptive personality disposition, or he could have woven together the language of marginal economics and the flip-flop of psychoanalytic theory to produce an irrefutable thesis. But this is not his style. He disregards structural-functional interpretation and stays close to his data.

He expresses an aversion to the direct questionnaire assessment of needs and chooses to rest his case on a content analysis of an interview in which the respondent reported how he felt and acted when he saw his name in print, had someone step in front of him in line, etc. The "need" interviews were supplemented by case histories, and an eight card TAT. He makes two analyses, one quantitative, based upon pooled ratings of needs, and the other qualitative, based primarily upon life history materials. The presentation of these analyses was, unfortunately, influenced by the requirements of trade book

publication, hence much of the quantitative detail one might seek was not present although it may be presumed that it would be available in part in the earlier articles of the author and his collaborators.

In the quantitative area, the lack of independence in the ratings of one individual on many needs and traits (and the consequent lack of independence between sets of correlations across the 25 couples) is not solved in this book. The gesture of considering the number of independent factors on pages 115 and 116 is interesting, but one must protest that if there are six factors in the cluster, then the use of essentially a two-factor scheme in the qualitative portions of the book is not indicated! Or, on the other hand, if two factors are adequate, then even the corrected degrees of freedom are overestimated.

The qualitative typology of Ibsenian (husband dominant and nurturant, wife opposite), Thurbrian (husband submissive and nurturant), Master-Servant Girl (husband dominant and receptive), and Mother-Son (husband submissive and receptive) is reported to classify satisfactorily 16 of the 25 cases. It is to the author's credit that he recognizes the nine exceptions so straightforwardly. It would have been helpful if the author had reported individual factor scores so that the cases discussed could have been directly related to the quantitative analysis.

One is impressed with the degree to which it is the recollections the subject has of his parents (as he knew them between, say, 6 and 18) which either directly, or as a counter-process, shapes his needs. However, when the materials presented are obtained from both husband and wife, they frequently show a distortion of the perception of a spouse in line

with the subject's needs. One therefore is at a loss to know how much to discount the patness of reported fit between parent and subject since independent parent observation is not available.

The anticipatory discussion of the social origins of needs in Chapter 4 is wisely selective in its emphasis upon internalization, ego-ideals, and adjustment to age and sex groups. The men in the author's sample were frequently concerned because they were not aggressive enough, and both the men and women tended to wish they could participate more fully in sexual activities. The author doubts that lower class couples would have comparable anxieties about sexuality or aggression. It is to be regretted that a replication among lower class couples is not contemplated. Further information on class and induced need structure would more fully establish the author's thesis as well as check current theories which hold that gang fighting and related deviance arise from inadequate male sex role models in female-based lower class families.

Linton Freeman's chapter on mate-selection in non-western societies contributes competently to the development of the argument. Martha Winch states clearly the implication of the theory for marriage counseling in the last chapter. Those, like this reviewer, who are reluctant to sanction the social application of theories which are to this degree in pilot form must grant that this calculated risk may result in the mobilization of the considerable resources of the marriage counseling field in the further test of the theory.

The originality of this work deserves recognition within the family field and it may have even broader implications for assessing role interdependence in other stable group relations.

FRED L. STRODTBECK

University of Chicago

BOOK NOTES

Freedom, Virtue and the First Amendment. By WALTER BERNIS. Baton Rouge: Louisiana State University Press, 1957. xiii, 264 pp. \$4.00.

Mr. Berns deprecates the 17th century and the rise of so-called liberal thought. Until this catastrophe, "the purpose of government was not to preserve freedom but rather to establish justice," and "the essential purpose of law was . . . to prevent the problem of freedom arising in the first place . . . through moral education" (pp. 240, 241). He doubts that "law can reasonably look upon the freedoms of the First Amendment as rights" (p. vii) and insists that "the very notion that the citizen possesses rights against government . . . makes no sense"

(p. 67). It will be seen from this that whereas he deals largely with the Supreme Court's interpretation of the First Amendment, his real quarrel is with the philosophy behind that Amendment.

The author argues his points with ability, enthusiasm, and learning. He is at his best in tracing the rise and fall of the clear and present danger test (Chapter IV), which of course lends itself to caustic comment, and in analyzing the Dennis case, where "the five opinions . . . reveal subterfuge, confusion, anger, and sorrow, but hardly a flickering of wisdom" (p. 206). He concludes that "wise decisions in First Amendment cases cannot be had without doing what our liberal courts refuse to permit: distinguishing between good and evil" (p. 47).

Although the Preface insists (p. viii) that the book "offers no rules of law with which the Supreme Court can settle First Amendment disputes," it comes pretty close to proposing a formula on at least two occasions. On page 125 we are told, "The plain fact is that not all free speech is good speech. Which means that freedom of speech is not always a sound or just public policy. Since, however, absolute suppression . . . is an evil . . . and since one suppression can easily lead to another, the . . . wise policy . . . is to permit as much even of the bad speech as can be tolerated, on the basis that its influence will be negligible. Thus, Terminiello should be permitted to speak as long as his speaking does not lead to street riots or to widespread acceptance of his ideas." And in the closing chapter we read: "If we continue to insist that the First Amendment is more than a prohibition of previous censorship, its only reasonable definition would read: Congress shall extend freedom to all good speech. For the guiding question should not be, from whom and under what circumstances is a 'right' to free speech withdrawn, but rather, *to whom is the freedom of speech granted*. The most immediately forthcoming answer to this question is: freedom is extended to those we can trust not to misuse the privilege" (p. 251). All italics are his.—J. A. C. GRANT

Slavery. By C. W. W. GREENIDGE. New York: Macmillan Co., 1958. 235 pp. \$4.75.

Slavery is both a social history of man's struggle to eliminate human bondage and a propaganda piece designed to keep the public aware of the continuing need for the kind of work performed by the Anti-Slavery Society (UK). It is written by a member and former officer of the Society who has also been active in the United Nations on the *Ad Hoc* Committee on Slavery. The dual objectives of the study are accomplished by careful documentation drawn principally from the reports of both the League of Nations' and the United Nations' Study Committees and Missions, and from the archives of the Anti-Slavery Society. Although the book makes an urgent plea for continued concern with slavery in today's world, the case is made with such scholarly restraint and understatement as to be almost ineffective propaganda.

For the student of society, this book gives an excellent world view of an aspect of human relations which it is difficult to believe still flourishes. Mr. Greenidge describes four forms of human bondage in the regional settings where they are prominent: the old chattel slavery found in many of the countries of the

Arab world; the "sham adoption" of children in the Far East; the "sale" of women into marriage in tribal Africa; and the serfdom of Latin America. Important to the purposes of the book and of interest to the student of public trusts (Slave Charities, bequeathed long ago) is the discussion of the diversion to uses other than the fight against slavery of funds originally designed to ransom slaves. The author's conclusion is that much remains to be done and can be accomplished through legislation, land reform, and the careful supervision of children in all the countries of the world. Toward these ends both public interest and international financial aid are essential.—JOHN LOBB

Los Factores Demográficos en la Planeación Económica. By DANIEL MORENO. México, D.F.: Cámara Nacional de la Industria de la Transformación, 1958. 329 pp. No price indicated, paper.

Lawyer Moreno gives a convincing picture of major demographic factors that need to be considered by economic planners in Mexico. He estimates the 1958 population of his country at 32,500,000—almost double that in 1930. This increase is accounted for by a reduction in the death rate to an average of 15.6 per thousand in 1950–53, which was then about one-third of the birth rate.

The rural population was 80 per cent in 1910, 57 per cent in 1950, and is estimated at 50 per cent for 1960. Where the percentages residing in places with less than 2,500 persons are high, as in southern Mexico, technology is backward and the level of living low.

From 1930 to 1950 Mexico City and immediate environs increased from about one-third of a million to almost three millions, from 2½ per cent to 12 per cent of the total population. This degree of concentration of population and of wealth (40 per cent of all automobiles in the country are registered in the Federal District) "begins to be harmful for the rest of the Republic" (p. 87).

The latest estimate on the number of illiterates in Mexico is ten million. During recent years, Moreno concludes, the literacy campaign "has diminished in intensity" (p. 313).

This is the best available compendium of current demographic data for Mexico.—NORMAN S. HAYNER

Mundurucú Religion. By ROBERT F. MURPHY. Berkeley and Los Angeles: University of California Press, 1958. iv, 146 pp. \$3.00, paper.

The Mundurucú, a river-dwelling people in Brazil, have been involved in the rubber-extrac-

tion industry since the 1870s. At present they show wide variation in degree of assimilation to Brazilian society.

The present monograph is a by-product of Murphy's research on the impact of rubber-extraction on Mundurucú society. The author's thesis, soundly documented, is that changes in Mundurucú religion are better understood as consequences of social and economic changes than as results of missionary or proselytizing activities.

Detracting from an otherwise workmanlike monograph is the offhand treatment accorded to method. Descriptions of the field roles of the author and his wife, types of informants, information-gathering techniques and the like are either omitted or interspersed as casual comments throughout the text.

The volume is a useful contribution both to the ethnography of South American Indians and to the sociology of religion.—EDWARD WELLIN

PUBLICATIONS RECEIVED

(Listing of a publication below does not preclude its subsequent review)

- AGARWALA, A. N. and S. P. SINGH (Editors). *The Economics of Underdevelopment*. New York: Oxford University Press, 1959. vii, 510 pp. \$4.65.
- (AMERICAN BAR ASSOCIATION SECTION ON INTERNATIONAL AND COMPARATIVE LAW). *The Rule of Law in the United States: A Statement by the Committee to Cooperate with the International Commission of Jurists*. The Hague: International Commission of Jurists, 1958. 108 pp. No price indicated, paper.
- ANTONI, CARLO. *From History to Sociology: The Transition in German Historical Thinking*. Foreword by BERNEDETTE CROCE. Translated from the Italian by HAYDEN V. WHITE. Detroit: Wayne State University Press, 1959. xxviii, 249 pp. \$4.50.
- BARCLAY, DOROTHY. *Understanding the City Child: A Book for Parents*. New York: Franklin Watts, 1959. xii, 273 pp. \$4.95.
- BASCOM, WILLIAM R. and MELVILLE J. HERSKOVITS (Editors). *Continuity and Change in African Cultures*. Chicago: University of Chicago Press, 1959. vii, 309 pp. \$7.00.
- BEE, LAWRENCE L. *Marriage and Family Relations: An Interdisciplinary Approach*. New York: Harper & Brothers, 1959. xii, 500 pp. \$5.50.
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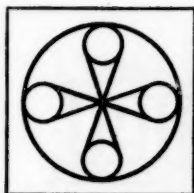
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